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Abstract: This paper studies the economic effects on re-employment and program substitution behavior among elderly displaced workers who exogenously lose eligibility for their early retirement option. We use detailed Norwegian matched employer-employee data containing information on bankruptcy dates and individual-level wealth, income, pensions and social security benefits. Our empirical strategy employs a regression discontinuity design, as job displacement before a certain age cut-off results in losing eligibility for early retirement benefits between ages 62–67 years in Norway. We find that re-employment rates are indistinguishable between workers who just retain eligibility for early retirement benefits and those who just do not. Meanwhile, those who lose eligibility offset 69% of their lost benefits through take-up of other social security benefits, where 51% comes from disability insurance and 13% from unemployment insurance. Our findings are particularly policy relevant as tightening of age-limits for old-age pensions is on the agenda in several OECD countries, while current economic hardship throughout the region may lead to increased job displacement for elderly workers.

Keywords: early retirement, job displacement, labor supply, benefit substitution, social security
JEL codes: H55, I38, J14, J26, J65

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1 Introduction

Pension entitlements can be affected by interrupted labor market careers, and pension systems are typically not designed to fully offset shocks affecting individual work careers (OECD, 2015). Retirement income is tightly linked to past earnings history, and unwilling displacement from the labor market may therefore lead to a negative wealth shock in terms of lost pension entitlements. In many OECD countries, late-career job displacement may even lead to individuals losing the ability to retire early or at the same terms as full-career workers.[1] While late-career job displacement already has severe implications for individual welfare, the loss of pension entitlements adds an additional element of reduced individual welfare that may have implications for job-seeking effort or enrollment onto other social security programs.

The main contribution of this paper is to assess the economic implications of access to early retirement benefits for elderly displaced workers. In particular, we study (i) the adverse effects on re-employment rates, (ii) benefit substitution onto other social security benefits and (iii) the associated implications on policy and welfare. A sharp eligibility criterion in the Norwegian early retirement program (AFP) facilitates our study.[2] Before 2011, workers in private sector firms covered by the AFP scheme could claim early retirement benefits from the age of 62, but in order to be eligible, workers had to be employed by the firm at the date of claiming. A job displacement before an individual cut-off date therefore implied that the individual did not qualify for AFP benefits, provided between ages 62–67 years. This allows us to employ a regression discontinuity design to study causal effects of early retirement provision for various outcomes, by comparing workers who lost their job just too soon to be eligible versus workers who just retained their eligibility. To identify job displacements, we use data on bankruptcies among Norwegian private sector firms between 2001–2010 which helps avoid potential endogeneity problems of workers voluntarily leaving a firm.[3] Combined with high-quality data on matched employer-employee relationships and take-up of various social security benefits from tax registers, this allows us to study effects of early retirement eligibility on re-employment rates, earnings, and benefit substitution between ages 62–67. We focus in particular on substitution towards disability insurance (DI) and unemployment insurance (UI). Furthermore, we explore the welfare implications and the financial costs for the state.

Our main empirical findings can be summarized by the following conclusions. First, we do not find evidence of eligibility for early retirement harming re-employment rates among workers in our sample. We estimate that re-employment rates among workers who are displaced just before becoming eligible for AFP are only 2 percentage points lower than among workers who are displaced just after becoming eligible, with the point estimate being statistically insignificant. Our corresponding estimate on labor market earnings is similarly small and insignificant and suggests that early retirement eligibility decreases labor market earnings between ages 62–67 by $5,600, or about 9 percent. Second, we find clear evidence of program substitution, and in particular increased enrollment onto the DI program among ineligible workers. The fraction of workers who are displaced just before becoming eligible for AFP and

[1] Some examples are Austria, Estonia, Hungary, Israel, Korea, Norway, the Slovak Republic, Sweden, Chile, Mexico and Germany (OECD, 2015).

[2] The early retirement program in Norway is known by its acronym AFP from Norwegian "Avtalefestet pensjon".

[3] For job displacements due to bankruptcies, an additional rule referred to as the "52-week-rule" pushed back this threshold to 61 years of age, plus the standard notice period, which may be some period from 1–6 months depending on tenure and age of the worker. This means that the relevant cut-off for workers experiencing a bankruptcy is individual-specific and may be some time between 60 years and 6 months to 60 years and 11 months. The details of this will be outlined in Section 2.

claim DI before reaching the general retirement age of 67 years is about 48 percent, compared to just 12 percent among workers who just retain AFP eligibility. The increase in DI claiming of 36 percentage points among the ineligible is highly statistically significant. We further estimate that of the $61,600 in lost AFP benefits among the ineligible, $42,500 is replaced by non-pension public transfers, where about $31,400 is increased take-up of DI benefits and $7,700 is increased take-up of UI benefits. This is equivalent to a replacement of 69 percent of the lost benefits with other non-pension social security benefits, where about half of the lost AFP benefits are replaced with DI.[4] Third, there is substantial heterogeneity in benefit substitution behavior among workers in our sample. Benefit substitution is largest for workers with low earnings, workers with low educational attainment and workers in the manufacturing industry. Fourth, we find that the increase in public expenditures of providing early retirement benefits is modest. Our point estimate suggests that the increase in overall costs amounts to $7,200 for each worker per annum, and at the 95% confidence level, our findings suggest that the increase is at most $16,400.

We emphasize that our findings should be interpreted with some caution. As the composition of workers in the private sector with access to AFP is heavily skewed towards male workers (more than 75 percent), and bankruptcies occur more commonly in the manufacturing industry (about 68 percent of all bankruptcies) where workers typically have low educational attainment, our main findings are mainly driven by workers with these characteristics. We do not find evidence of increased take-up of DI for workers in non-manufacturing professions whose bankruptcy occurred before their individual age cut-off, but we do find substitution towards UI for these workers. Moreover, we do not find distinguishable differences between workers with high educational attainment who reach the eligibility threshold and those who do not.

To investigate the welfare implications of early retirement provision for elderly displaced workers, we assess disposable income among workers in our sample who were eligible for AFP and those who were not. We do not find clear evidence of disposable income being higher among the eligible on average, with a statistically insignificant point estimate of about $3,800 per annum, or about 12 percent higher than among the ineligible. However, this exercise does not necessarily capture the full picture, and in particular whether some ineligible individuals are significantly worse off. Therefore, we extend our exercise and investigate distributional impacts in a standard Imbens & Rubin (1997) framework, and assess the distribution of disposable income depending on eligibility status. While we do find evidence of disposable income being more dispersed among the ineligible, the difference in the lower part of the distribution is small. This suggests that few individuals in our sample were significantly worse off when being ineligible for early retirement benefits, and that most ineligible individuals who were not re-employed got some type of social security benefit.

We believe our analysis is of general interest for three main reasons. First, economic hardship throughout the OECD may lead to increased job displacements and decreased labor demand, and in particular for elderly workers who are usually less attractive hires.[5] Second, many countries have implemented early retirement schemes to provide more flexible withdrawal opportunities from the labor market and to reduce enrollment onto other social security programs, but these programs have also turned out to be very costly. We are able to shed light on a particularly large shock to early retirement

[4]Regarding the very high substitution onto DI, we emphasize that on average about 23 percent claim DI at some point between ages 62–67 in the population, while among those who experience a late-career job displacement and at the same time do not reach eligibility, about 48 percent claim DI at some point between ages 62–67.

[5]See e.g. Heyma *et al.* (2014); Vigtel (2018).

entitlements, as losing eligibility for AFP leads to the loss of the entire early retirement option, or the equivalent of five years of benefits. We are also able to account for the outcomes in the entire early retirement period for our sample individuals, meaning that we can fully account for the employment effects in the period of interest and the potential program substitution. Third, many countries are debating whether parts of the social security system in general must undergo reforms to uphold fiscal sustainability. This may lead to the use of prescriptions such as eligibility tightening or benefit cuts, prescriptions to which we provide evidence to policymakers' knowledge about potential gains and harms.

Our paper is primarily related to the literature focused on the effects of extended UI for elderly workers. Closest to our paper is a few studies which have shown that extended UI benefits discourages job searching and prolongs unemployment spells, and may even bridge the gap to retirement.[6] Inderbitzin *et al.* (2016) showed that extended UI have strong effects on labor market exit through early retirement, and increased exit through the DI channel. Kyyrä & Ollikainen (2008) used a reform in Finland which increased the eligibility age for extended UI from 53 to 55 and later in Kyyrä & Pesola (2020) from 55 to 57 to study the effects on early retirement and labor supply, respectively. Kyyrä & Ollikainen (2008) documented a decrease in early retirement from the first increase in access age, while Kyyrä & Pesola (2020) documented increased employment over the remainder of the working career, and no substitution onto other programs. In contrast to the literature on extended UI, our study consists of workers very close to the general retirement age. We contribute to this literature by studying effects of having the option to retire early, and thus exiting the labor market entirely, which we argue may have fundamentally different implications than extended UI spells.

Our paper is also related to the literature on early retirement programs and changes to the minimum legal retirement age on labor supply and program substitution (e.g. Geyer & Welteke, 2017; Manoli & Weber, 2016; Staubli & Zweimüller, 2013 among others). Their common finding is that increasing the retirement age increases employment, but evidence on program substitution is mixed. Hernæs *et al.* (2016) used a recent Norwegian reform of the pension system which gave workers more flexible withdrawal opportunities, while Johnsen *et al.* (2020) used the introduction of the Norwegian early retirement program, essentially studying a reduction in the legal retirement age. Their common finding is that workers tend to decrease take-up of DI benefits in response to greater flexibility of the retirement program. Vigtel (2018) showed, on the labor demand side, that decreasing the minimum legal retirement age in Norway for a subset of workers leads to risk-averse firms becoming more willing to hire senior workers. While most of these studies are focused on the spillover effects between two programs or their employment effects, our paper broadly investigates the spillover effect onto the entire spectrum of social security programs that the elderly workers may be eligible for. In that sense, we contribute to the literature by broadening the scope of program substitution.

Another broad branch of the literature is focused on the effects of tightening policies regarding eligibility for social benefits and their effect on employment rates and program substitution. Borghans *et al.* (2014) studied how stricter criteria for access to DI in the Netherlands affected enrollment onto other social insurance programs, and found that individuals disqualifying for DI offset about 30 percent of the lost benefits in take-up of other social benefits. Similarly, Karlstrom *et al.* (2008) found that stricter eligibility criteria for DI in Sweden increased take-up of UI and sickness benefits, but that it did

[6]While this literature is often interested in the push and pull factors of UI systems for older workers into unemployment (e.g. Tuit & van Ours, 2010 and Baugelin & Remillon, 2014), we do not explore this margin in our paper.

not influence employment rates. Staubli (2011) suggests that increasing the minimum age of relaxed DI access in Austria only had a slight positive effect on employment rates, but a significant decline in DI enrollment. Our study echoes these studies regarding the importance of assessing program substitution when considering policy changes to social security programs.

Finally, our paper is related to an extensive literature on the effects of job displacement (e.g. Jacobson *et al.*, 1993; Lassus *et al.*, 2015; Marmora & Ritter, 2015; Ichino *et al.*, 2017; Huttunen *et al.*, 2018 among others). Common findings for these studies are large adverse effects on earnings and employment, both in the short and long run. Particularly relevant for our study is Bratsberg *et al.* (2013), who used data on Norwegian bankruptcies and showed that a large fraction DI claims can be attributed to job displacements. They found that non-participation in the labor market is significantly affected by exogenous changes in employment opportunities. Marmora & Ritter (2015) found that unemployment late in workers' careers affects retirement timing, and that the effect is stronger once the workers become eligible for social security benefits. Recently, Ichino *et al.* (2017) showed that old and young workers face similarly large displacement costs in terms of long-run employment, but older workers lose considerably more initially and gains later. While our study does not primarily focus on the effects of job displacement, we show how an outside option for displaced workers affects re-employment rates and enrollment onto social security programs.

The remainder of this paper is organized as follows. First, we present an overview of the Norwegian early retirement program, and briefly provide an overview of the related public transfer systems in Section 2. Then, in Section 3, we present the administrative data that we use, while in Section 4 we lay out our empirical strategy. In Section 5 we present our main results. In Section 6 we present a fuzzy RD as an extension of our main results. In Section 7 we assess the implications of our findings for policy and welfare. Finally, we conclude in Section 8.

2 Institutional setting

Our focus lies on elderly workers in private sector firms covered by the early retirement program (AFP) who experience a job displacement due to bankruptcy of the firm they work in. The institutional background information therefore includes an overview of the AFP program and the eligibility criteria including particular rules concerning firm bankruptcies. We also provide a brief overview of other social security programs that workers may be eligible for, and in particular the disability insurance (DI) program and the unemployment insurance (UI) program.

2.1 Early retirement (AFP)

The AFP program was introduced in 1988. For public sector workers, there has been full coverage since the introduction, while about half of private sector workers have been covered since the introduction, although the rate has increased somewhat over time. For private sector firms, membership is voluntary and requires a centrally negotiated collective pay agreement. For member firms, employees are enrolled regardless of their individual union memberships. The AFP is partially funded by the government, and partially funded through payments by member firms. Until November 2010, the AFP offered enrolled workers a full pension claim starting from age 62, whereas the normal retirement age through the National Insurance Scheme was 67 years.[7]

[7] When the system was first introduced, the minimum claiming age was set to 66 years, but the limit has since been reduced in four steps. The final reduction of the minimum legal claiming age happened in 1998, and all our possible claimants became

Eligibility criteria The AFP system imposed a sharp lock-in (or lock-out) mechanic. Workers had to be working in *the same firm* covered by the AFP for the last three years before claiming benefits, or in *any firm in the same sector* covered by AFP for the last five years with the last two years being in one firm.[8] Furthermore, the firm had to employ at least two workers not counting the owner of the firm. At the day of claiming benefits, the worker had to be employed by the firm, and the first possible claiming time is the beginning of the month after reaching age 62. Workers' salary had to be at least equivalent to approximately $\$10,000$ (in 2015 dollars) in annual earnings, with this firm being the worker's main employer. Finally, claiming AFP benefits could not be combined with claiming DI benefits.

There was an exception made in the case of mass-layoffs or bankruptcy. If the work relation was terminated because of either of these events, the worker retained the AFP membership for 52 weeks after the day of the incident plus the duration of the standard notice period. The standard notice period is governed by the Norwegian Work Environment Act and is a mapping based on tenure and the worker's age, where the shortest notice period is 1 month and the longest is 6 months.[9] This means that a worker who lost the job due to bankruptcy or mass-layoff essentially could retain the membership for up to 18 months after the incident.

Benefits levels The AFP benefit level was a mapping from the old-age pension benefit that the worker would receive from the National Insurance Scheme given pension claiming at age 67. The old-age pension benefit level received at age 67 was unaffected by claiming AFP. We provide a detailed overview of how old-age benefits were calculated in Appendix B. Additionally, claimants received an "AFP top-up", which was a flat rate of about $2,300.[10] Claimants were subject to a pro-rata earnings test on continued work above a very small tolerance level, essentially implying a marginal tax-rate on continued work close to 100 percent for those who claimed AFP. Average annual benefits amounted to about $24,000 in 2001 and approximately $27,000 in 2010. The average benefit levels were significantly higher for men than for women. In 2001, the average benefit for men was $27,000 and for women $22,000 while in 2010 the average benefit for men was $31,000 and for women $24,000.

2.2 Other social security benefits

Disability insurance For those deemed to have permanent reduced earnings capacity due to illness or injury, disability insurance (DI) benefits replaces parts of the past earnings that are lost due to the reduced capacity. This benefit may be partial, depending on the residual earnings capacity. To be eligible for disability benefits, an individual must be between 18–67 years old and have been a member of the National Insurance Scheme in the last three years before becoming disabled. Illness or injury must be the main reason why the earnings capacity has been reduced, appropriate vocational rehabilitation measures must have been completed and the earnings capacity must be permanently reduced by at least 50 percent.[11] In the time period we consider in this paper, the benefit level was equivalent to the old-age

eligible *after* this year, unifying our minimum legal claiming age for AFP to 62 years of age. The structure of the AFP, including some of the rules governing eligibility, was changed in 2011, a reform that does not affect our sample as workers in our cohorts spanning from 1939–1948 were entirely covered by the old rules.

[8] For instance, switching jobs between private and public sector firms just before retirement would lead to loss of eligibility for AFP benefits, even if both the private and the public firm were covered by AFP.

[9] The exact mapping from age and tenure to the notice period is displayed in Equation (2) in Section 4.

[10] In 2015 dollars. Throughout the paper, we measure monetary values in 2015 dollars given an average exchange rate of NOK/USD = 9.

[11] Under some criteria, DI may be given even though the earnings capacity is reduced by less than 50 percent; if the worker is currently on the work assessment allowance program, 40 percent is sufficient, and if the reduced earnings capacity is due to

pension benefit, and therefore almost equivalent to AFP benefits (the difference was equivalent to the "AFP top-up" of $2,300 per annum). Individuals allowed DI were subject to an earnings test implying a marginal tax rate of about 60 percent if earnings exceeded about $10,000.[12]

Unemployment benefits To be eligible for unemployment benefits, a person must be a registered job-seeker at the Norwegian Labour and Welfare Administration. A person whose working hours have been reduced by at least half, is a genuine job-seeker, a member of the National Insurance Scheme, a legal resident, and has had at least $15,000 of income in the previous calendar year or $30,000 combined over the past three calendar years may apply for unemployment benefits. If the pre-unemployment income exceeded $20,000, the recipient may receive unemployment benefits for up to 104 weeks, while if it was lower than $20,000, the longest period is 52 weeks. A recipient of unemployment benefits is entitled to 62.4 percent of the past earnings. The past earnings are either the last 12 months before unemployment, or the annual average of the last 36 months if this exceeds the former.

Other public transfers Besides disability and unemployment insurance, workers in our sample may also be eligible for various other social security benefits. One particularly relevant program for elderly workers is sickness benefits which is intended as replacement of income loss due to short-term sickness (up to one year) for workers engaged in employment who are members of the National Insurance Scheme. A full sickness benefit fully replaces the earnings in the past year. Although less relevant for elderly individuals than for prime-age workers, workers in our sample may also be eligible for temporary DI benefits. While the temporary DI program has undergone several changes during our sample period, the program's main intention has been to provide financial support in periods when the person is ill or injured but attempt to return to work. Temporary DI was provided for up to 1–4 years during our sample period for most individuals.[13] Additionally, individuals in our sample may also be eligible for a few less relevant benefits such as social assistance and child support.

3 Data and sample selection

In our empirical analysis we use data from two main sources that can be linked by unique and anonymized identifiers for every resident individual and employer. The main data we use is provided by Statistics Norway (SSB) and contains detailed information about individual characteristics and employer-employee relationships, including exact dates of each relationship. This allows us to construct monthly data on earnings and employment for each individual and firm. The employer-employee data also contains information on firm characteristics, including 5-digit industry codes and the exact date of bankruptcy (if such a date exists). Thus, we are able to identify individuals who work in firms experiencing a bankruptcy. Our second source of data is provided by *Fellesordningen for AFP*, and includes information about exact dates on each firm's affiliation to the AFP-scheme.[14] This allows us to identify

an approved occupational illness or injury, 30 percent is sufficient.

[12] Every dollar in earnings were earnings tested if earnings exceeded this threshold. After 2005, only the earnings above the threshold were earnings tested if the individual was allowed DI in 2003 or earlier.

[13] Before 2010, temporary DI consisted of three separate programs: Rehabilitation benefits (up to 1 or 2 years), occupational rehabilitation benefits (no upper time constraint) and time-constrained DI benefits (up to 5 years). In March 2010, these programs were replaced with the Work Assessment Allowance program that provided benefits to individuals for up to 4 years as a general rule.

[14] Fellesordningen for AFP is the largest private sector organization for AFP schemes and almost the entire market.

whether individuals are eligible for AFP based on their employment relationship which is crucial for our analysis. For our main outcome variables, we use annual data on earnings and social security transfers from reported tax-records (SSB). The data we use contains years 1999–2014.

The administrative nature of our data reduces the extent of measurement errors in income variables and employment relationships. Because individual employment affiliation and income variables are third-party reported (i.e. by employers and the tax authorities), the coverage and reliability are rated as exceptional by international quality assessments (see e.g. Atkinson *et al.*, 1995). Since administrative data are a matter of public record, there is no attrition due to non-response or non-consent by individuals or firms, and individuals can only exit these data sets due to natural attrition (death or out-migration).

3.1 Sample selection

In our empirical analysis, our main estimation sample considers workers aged 59–61 years when the firm experiences a bankruptcy. The upper age restriction is set to avoid selection bias. As workers in affiliated firms are eligible for AFP benefits from the age of 62, we ensure that individuals in our estimation sample have not yet made their decision to retire early. The lower age restriction ensures that we have roughly 18 months of bandwidth on each side of the cut-off in our RD analysis. A potential worry in our setting is that firms may lay off workers before the actual bankruptcy occurs. Another worry is that workers may anticipate that their job is at risk and leave early. To avoid such selection of workers, our main estimation sample includes those workers who were employed in a firm with AFP affiliation 24 months prior to the bankruptcy date of the firm. Thus, we also pre-determine worker and firm characteristics to this initial point in time (when workers are 57–59 years of age). While our estimates appear to be very stable across different specifications of when we pre-determine work affiliation, we test alternative samples of workers with pre-determined affiliation 12 months and 1 month before the bankruptcy dates of firms as robustness checks.

Additionally, we do the following sample restrictions due to the eligibility criteria of the AFP program presented in Section 2.1. One of the requirements states that individuals must work at least 3 consecutive years in the same firm with AFP affiliation. To be eligible for AFP at the age of 62, we therefore require that individuals started their employment relationship before the month of when individuals turned 59 years. We also require that the specific employment relationship was each individual's main employer (the one with the highest earnings) if the individual had more than one employer, as only the main employment relationship was considered for eligibility. Third, we require that individuals did not participate in the DI program, as recipients of this program were ineligible for AFP benefits. Fourth, we require that firms have at least 2 employees as workers were considered ineligible if there were no other employees at the firm. Fifth, we require that individuals worked at least 20 percent of a full-time position, which translates to roughly $10,000 in annual earnings to meet the final eligibility criteria for AFP.

Even though our data contains information on registered firm bankruptcies, some of the firms may get new owners and keep a share of the workforce, leading to few or no job displacements despite the original firm being bankrupt. As we are interested in workers who in fact do experience a job displacement, we therefore follow previous studies (see e.g. Jacobson *et al.*, 1993; Rege *et al.*, 2009; Huttunen *et al.*, 2011; Basten *et al.*, 2016), imposing a restriction on the fraction of workers (including younger workers not in our estimation sample) who from the month of the bankruptcy to 12 months post-bankruptcy work in the same firm. In our baseline specification, we set our threshold to 1/3 meaning that if more than 1/3 of all workers in the bankruptcy firm (excluding "self"[15]) work in the *same firm* 12 months after the bankruptcy, it is considered a "spurious bankruptcy" and the entire firm is dropped from our initial estimation sample. We do, however, include these firms in an alternative sample as a robustness check.

As our data spans from 1999, our main estimation sample includes bankruptcies in private sector AFP-firms during January 2001–November 2010 and cohorts 1939–1948.[16] This means that for firms with a bankruptcy occurring in 2001, our workers must be employed by the firm in 1999. As our data spans to 2014 we are able to follow individuals during the entire early retirement period until they reach the standard retirement age of 67 years.

3.2 Descriptive statistics

In Table 1 we present summary statistics for individuals aged 57–59 years who work in a private sector firm. The first two columns include our main estimation sample of individuals who worked in a private sector firm with AFP affiliation 24 months before the bankruptcy. The third and fourth columns include workers in bankruptcy firms without AFP affiliation which we use as a placebo sample in the empirical analysis. The fifth and sixth columns include workers who worked in private sector firms that did not become bankrupt, which we use as a comparison sample in our analyses.

There are some noteworthy differences between our main estimation sample of workers in bankruptcy firms with AFP affiliation and the other private sector firms that do not become bankrupt, particularly for industries. The firms in our estimation sample are far more likely to be in the manufacturing sector, while the workers are more likely to be male workers and have slightly lower earnings on average. Firms are also somewhat smaller compared to the other private sector firm. Otherwise, workers share fairly similar characteristics.

[15]For instance, the workers in a firm with 10 employees which ends up bankrupt is "spurious" if $n > (10-1)/3$ works in the same firm a year after the bankruptcy, where the one subtracted is "self".

[16]We restrict our attention to bankruptcies occurring before the 2011 Norwegian pension reform for two reasons; the reform changed the rules regarding eligibility and work incentives for individuals claiming AFP benefits. While workers in our sample could in principle become eligible for AFP benefits under the new scheme following the reform, individuals in our sample had to postpone claiming after the initial claiming month when turning 62 years, and had to be re-employed in a firm with AFP affiliation to satisfy the new eligibility criteria. Only 3% of our sample claim AFP benefits under the new scheme, compared to 37% claiming before the reform.

Table 1: Summary statistics of private sector workers aged 57-59 years

	Bankruptcy samples				Comparison sample	
	Main est. sample: AFP workers		**Placebo sample: Non-AFP workers**		**All private sector workers**	
Individual characteristics:	mean	sd	mean	sd	mean	sd
Age	58.0	(.84)	58.0	(.82)	58.0	(.82)
Fraction females	.23		.29		.33	
Fraction married	.75		.72		.76	
Years of education	10.8	(1.7)	11.2	(2.2)	11.4	(2.3)
Number of children	2.0	(1.2)	2.3	(1.1)	2.2	(1.1)
Wealth ($1,000)	89	(95)	94	(107)	119	(123)
Labor market characteristics:						
Monthly earnings ($1,000)	4.1	(1.8)	3.9	(2.1)	4.9	(2.3)
Fraction full time employment	.91		.87		.87	
Tenure (years)	8.8	(8.7)	6.1	(6.8)	10.9	(8.9)
Number of employees	84	(127)	11	(13)	157	(315)
Fraction receiving sickness benefits	.11		.11		.08	
Local DI rate	.10	(.03)	.10	(.03)	.10	(.03)
Local unemployment rate	.02	(.01)	.02	(.01)	.02	(.01)
Industry (%):						
Primary sector	1.2		3.1		4.3	
Manufacturing	68.1		18.8		30.0	
Construction	10.3		14.0		8.5	
Wholesale retail and trade	13.3		37.6		25.8	
Transportation and storage	1.2		7.5		9.2	
Scientific and legal activities	1.2		5.1		6.5	
Other	4.7		14.0		15.7	
Number of firms	177		511		48,451	
Number of individuals	339		591		141,122	

Notes: Bankruptcy samples include individuals aged 57–59 years who work in a private sector firm 24 months before the firm's bankruptcy date. Comparison sample includes individuals aged 57–59 years who work in a private sector firm (excluding bankruptcies). All samples include firms with at least 2 employees, individuals not on disability insurance, cohorts 1939–1948 and years 1999–2008. Firm must be each individual's main employer (with the highest earnings if more than 1 employer). Local DI and unemployment are measured at the municipality level. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

4 Empirical framework

This section first presents the assignment rule that creates local random variation in eligibility for early retirement (AFP). We then present the regression discontinuity design that we use to identify effects of early retirement eligibility and discuss threats to identification.

Assignment variable As our proxy for job displacements comes from bankruptcies, our assignment variable is based on the age of individual i at the time of the bankruptcy of the firm. As explained in Section 2.1, individuals are in normal cases eligible for AFP from the age of 62, but in the case of bankruptcies, workers are granted an additional 52 weeks plus the individual notice period. Hence, our assignment variable (measured in months) is defined as:

$$a_i = age_i - (61 - NP_i) \tag{1}$$

where age_i is individual i's age at the bankruptcy date and NP_i is the *notice period* (in months) of individual i, which is governed by the Norwegian Work Environment Act, according to:

$$NP_i = 1 + T_{i,5} + T_{i,10}(1 + \mathbb{I}_{i,50} + \mathbb{I}_{i,55} + \mathbb{I}_{i,60}) \tag{2}$$

where $T_{i,y}$ is a dummy equal to one if individual i has at least y years of tenure and $\mathbb{I}_{i,\bar{a}}$ is a dummy equal one if individual i is at least as old as age $\bar{a}$. The relationship implies that individuals in our sample have a notice period of 1–6 months depending on age and tenure. If a_i is positive (negative), then the firm went bankrupt sufficiently late (too early) and individual i is initially eligible (ineligible) for AFP benefits.

4.1 Regression discontinuity design

In our RD design, assignment to eligibility is a deterministic function of the assignment variable a, the age at bankruptcy including each individual's notice period as defined in Equations (1) and (2). Individuals are initially eligible for AFP if $a \geq 0$. The regression model for our reduced form RD model can be summarized by the following equations:

$$y_{it} = \alpha_l + f_l(a_i) + \delta X_{it} + \varepsilon_{it} \qquad \text{if} \quad a_i < 0 \tag{3}$$

$$y_{it} = \alpha_r + f_r(a_i) + \delta X_{it} + \varepsilon_{it} \qquad \text{if} \quad a_i \geq 0 \tag{4}$$

$$\beta = \alpha_l - \alpha_r \tag{5}$$

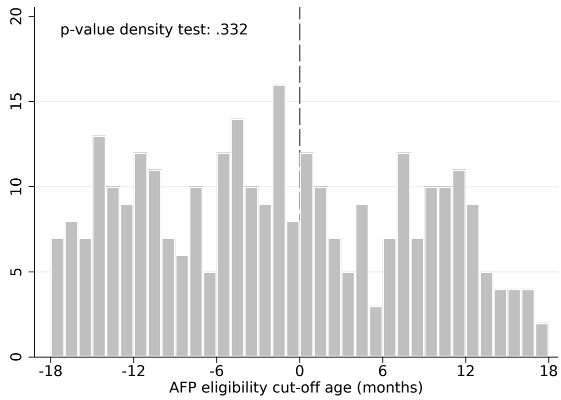
where y_{it} denotes the outcome of individual i at time t, X_{it} is a set of covariates, ε_{it} is the error term and f_l and f_r are unknown functional forms of the assignment variable on each side of the cut-off respectively. The reduced form RD estimate is given by β, the difference between the intercepts of each side of the cut-off.

In our baseline specification, we follow Lee & Lemieux (2010) and use a local linear regression with separate linear trends and a rectangular kernel density on each side of the cut-off. While we consider multiple outcome variables in our analyses, we keep our bandwidth fixed in our baseline specifications. Although different outcomes have different optimal bandwidths, we choose a bandwidth of 12 months (of age) which is in the neighborhood of the optimal bandwidth suggested by Imbens & Kalyanaraman (2012) for two of our key outcome variables AFP benefits and (total) social security benefits. We also show that our estimates are relatively stable to bandwidth selection in Section 5.4.

4.2 Threats to identification

The validity of our RD design requires that individuals are not able to precisely manipulate the assignment variable, which in our setting is their age at the bankruptcy date. As individuals cannot manipulate age, the only possible way to manipulate the assignment variable is manipulation of the bankruptcy date itself. While we consider this is highly implausible, we carry out the standard validity checks for RD designs. Figure 1 shows the distribution of the assignment variable around the cut-off. Because our assignment variable is discrete, we follow Frandsen (2017) and perform a formal statistical test for bunching on either side of the cut-off. Reassuringly, the test is unable to reject the null of no bunching.

Figure 1: Distribution of eligibility age around cut-off



Notes: The figure shows the distribution of age (in months; defined as in Equation (1)) around the individual AFP eligibility cut-off. P-value is calculated using the discrete density test of Frandsen (2017). The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

If individuals are unable to manipulate the assignment variable, any pre-determined covariate should have the same distribution on either side, close to the cut-off. As a formal test, we run RD regressions with our baseline specifications on worker characteristics as the dependent variable, each measured 24 months prior to the bankruptcy. The point estimates and standard errors are reported in Appendix Table A.1. We also present these results graphically in Appendix Figure A.1. Reassuringly, key covariates such as monthly earnings, tenure, and the number of employees in each firm appear smooth around the cut-off and are insignificant at all conventional levels. One exception is the local DI rate (measured at the municipality level) which is significant at the 5% level. However, based on the large number of covariates that we consider, the probability of observing changes in one covariate around the cut-off is quite large. Additionally, the correlations between the local DI rate and the outcome variables we consider are very small and close to zero. When we perform a joint test for all covariates, we cannot reject the null of no manipulation at any conventional level as reported in Appendix Table A.1.

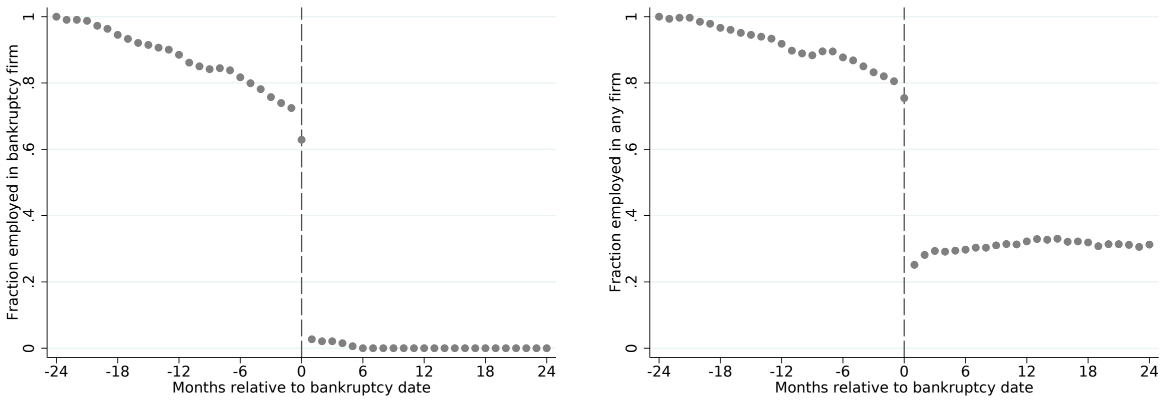
4.3 Interpretation of estimates

While a significant share of the workers are indeed displaced when their respective employer becomes bankrupt, not everyone is displaced at this point in time. As our main estimation sample consists of workers 24 months prior to the bankruptcy, some workers may be displaced or leave the firm for other reasons before the actual bankruptcy. While we impose this restriction to avoid selection, workers may still lose eligibility for AFP despite being initially eligible. Additionally, some workers may not be displaced at all as new owners may keep a share of the workforce in the event of a takeover, while other workers may become re-employed by a different employer. These individuals may become eligible for AFP at a later stage despite being initially ineligible. While we cannot perfectly distinguish between the firms that get new owners ("takeover firms") and other firms, we can investigate the overall employment rates around bankruptcy date of the initial employer.

Figure 2 shows the monthly employment rates for our main estimation sample of AFP-workers around the bankruptcy of the firm. In panel 2a, we plot the fraction of workers employed by the bankruptcy firm. While everyone was employed 24 months prior to bankruptcy by construction, just over 60 percent of workers were still employed by the firm in the month of bankruptcy. This indicates that a significant share of workers either left early or that the actual lay-off occurred before the bankruptcy date. There were very few who were still employed by the firm in the months after bankruptcy. In panel 2b, we plot the fraction of workers who were employed by any firm around the bankruptcy date of the original firm. Around 80 percent of workers were still employed in the month of bankruptcy, while around 25 percent were employed in the month after bankruptcy. This suggests that a substantial share of workers were re-employed either by new owners of the bankruptcy firm or by a different firm, and may gain eligibility for AFP despite being initially ineligible.

Figure 2: Employment around bankruptcy date

(a) Fraction employed by bankruptcy firm **(b)** Fraction employed by any firm



Notes: The figures show the fraction of individuals employed by bankruptcy firm (left) and any firm (right) relative to the month of bankruptcy. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

For perfect identification of exogenous loss (or gain) of access to AFP, we would ideally want to observe exogenous shocks to eligibility directly. However, we can only observe the age related to the day of the bankruptcy serving as an instrument for eligibility. While we are able to construct a measure of eligibility based on the various criteria, we cannot observe actual eligibility for AFP directly. It is also not clear how to define eligibility for AFP in our setting as workers who are initially ineligible may regain eligibility at a later stage if they become re-employed in a covered firm. In our main empirical approach, we therefore report reduced form estimates from the RD model outlined in Equations 3 and 4 which yields the intention-to-treat effect (ITT) of optional early retirement. These estimates can be interpreted as the effect of being initially eligible for AFP based on employment status two years prior to bankruptcy and can be considered as lower bound estimates of optional early retirement.

In an attempt to quantify the effect of optional early retirement, we use an alternative RD model where we use age at bankruptcy as an instrument for our constructed eligibility measure in a fuzzy RD approach. Under certain assumptions, this approach yields the local average treatment effect (LATE), that is the average effect of having the option to retire early for compliers in our sample.[17] While we are concerned about measurement errors in our treatment variable in particular, this approach is useful for better understanding of the effects of having the option to retire early. We report results from our fuzzy RD approach in Section 6.

5 Main results

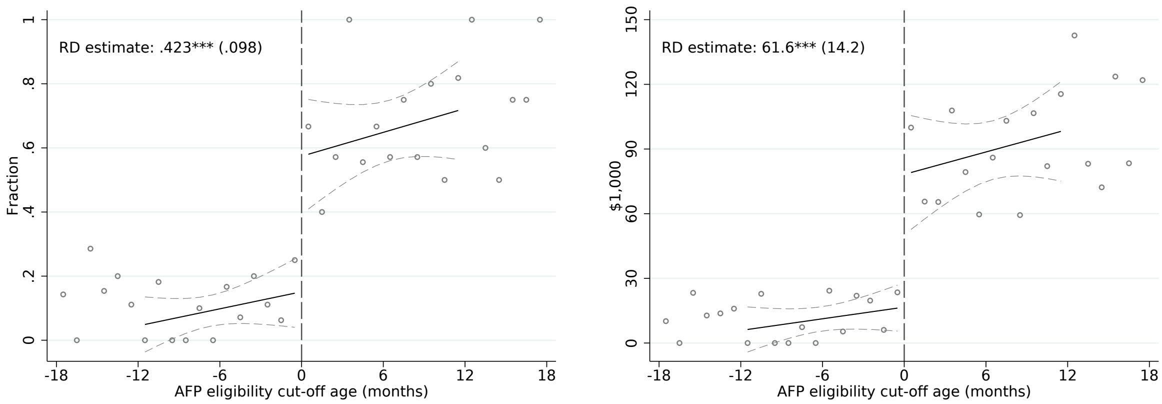
We now turn to our main results. First, we present the direct effect on take-up of AFP benefits from reaching the individual cut-off date before the bankruptcy occurs. We then turn to investigating the effects on subsequent employment, and finally explore whether the loss of eligibility for early retirement benefits induces benefit substitution toward other public transfer programs.

5.1 Direct effects on early retirement

Figure 3 illustrates two measures of the magnitude of the direct treatment effect: AFP claiming (panel a), which is a dummy equal to 1 if individuals have claimed AFP benefits at some point between ages 62–67, and AFP benefits (panel b), which is the cumulative take-up of benefits between ages 62–67 (in $1,000). The left-hand side observations consist of individuals who lose their job before reaching the eligibility cut-off, and thus lose their AFP benefit from that particular firm. However, they might recover the lost benefit by extending their working career or by leaving the firm early and find a new job. Those on the right-hand side are certain to fulfill the eligibility criteria if they are still employed by the firm when the bankruptcy occurs. The closer to the cut-off, the shorter the time-period for which the individual may claim AFP. Those who are just above the cut-off have to claim AFP in the month after they turn 62 years which is the first month they can claim AFP, and the last month they are considered as engaged in employment by the bankruptcy firm.

Figure 3: Graphical evidence of AFP benefit take-up between 62–67 years of age

(a) AFP claiming　　　　**(b)** AFP benefit take-up ($1,000)



Notes: The figures show the fraction of individuals with some AFP benefit take-up (a) and AFP benefit take-up in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. AFP benefits are measured in 2015 dollars (NOK/USD = 9).

[17]In our setting, the compliers are the workers who become eligible for early retirement because their age is above the eligibility cut-off.

The figures show a visually clear discontinuity at the threshold for the two measures of the magnitude of the direct treatment. Using our RD strategy, we estimate an increase in AFP claiming of about 42 percentage points among individuals who worked in firms that experienced a bankruptcy just after reaching the individual threshold. Equally, we estimate that these individuals claim about $61,600 more total AFP benefits. The estimates suggest that our treatment had a significant impact on the displaced workers' ability to retire early with an AFP benefit.

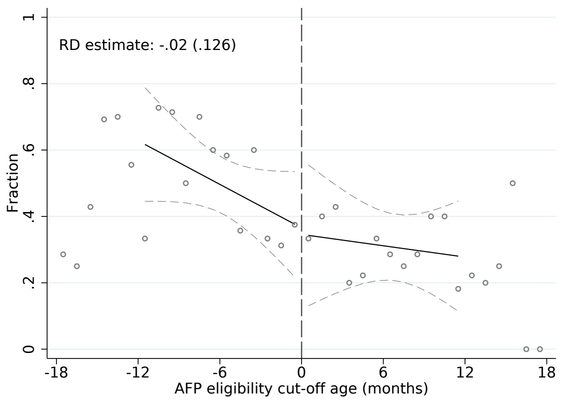
5.2 Effect on subsequent employment

We now ask whether initial AFP eligibility had an impact on re-employment rates and labor market earnings. Theoretically, those who lose eligibility should be induced to extend their working career to redeem some of the lost pension benefits at the expense of foregone leisure which becomes costlier. At the same time, individuals may have a hard time finding a new job as they are relatively close to the standard retirement age of 67 years. Local labor demand could also be an important factor.

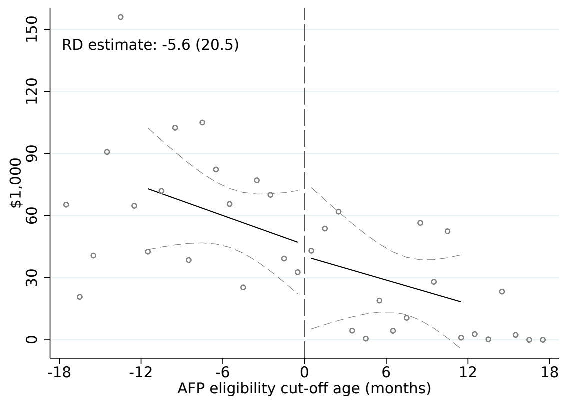
Visually, Figure 4a shows that we are unable to detect a discontinuity around the cut-off in terms of employment at the extensive margin between ages 62–67. Similarly, Figure 4b shows that we cannot distinguish between labor market earnings for individuals on either side of the cut-off, with a negligible point estimate of $5,600 which corresponds to about $1,100 in annual earnings. We observe a downward slope in both figures, consistent with the fact that those who are further to the right are older workers at the time of the bankruptcy and thus closer to the standard retirement age.

Figure 4: Graphical evidence of employment at the extensive margin and labor market earnings between 62–67 years of age

(a) Ever employed



(b) Labor market earnings ($1,000)



Notes: The figures show the fraction of individuals ever engaging in employment (a) and the unrestricted means for each age-bin of labor market earnings in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

We report regression results for the two outcomes in Table 2. The first column reports results of our main specification without controls. In the second column, we report results where we include the pre-determined covariates in Appendix Table A.1 as control variables and year fixed-effects. The inclusion of control variables barely moves our estimates which is reassuring as the pre-determined covariates should have the same distribution on either side of the cut-off. We also report means and standard deviations of the initially ineligible workers (i.e. the workers to the left of the cut-off) and of our comparison sample of all private sector workers in columns 3 and 4, respectively. Our results indicate that workers who lose eligibility for early retirement benefits because of job displacement are either unwilling to, or possibly unable to redeem parts of the lost benefits through re-engaging in the labor market. While this may be surprising from a theoretical point of view, a possible explanation could be that workers could offset some of the lost benefits if they are eligible for other types of social security benefits such as unemployment benefits before they reach the standard retirement age. We investigate this hypothesis in the next section.

Table 2: Effect of initial AFP eligibility on employment and labor market earnings between 62–67 years of age

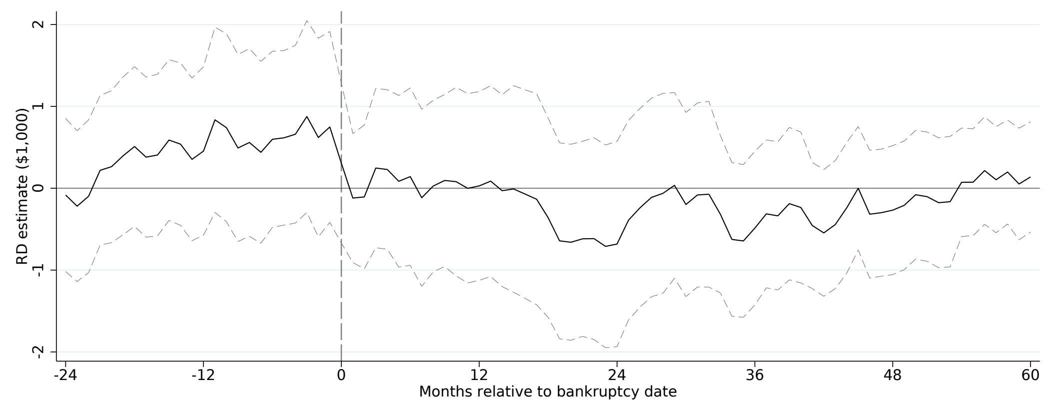
			Mean [SD]	
Outcome:	RD estimate (ITT):		Initially ineligible	All private sector workers
Ever employed	-.020	-.018	.492	.808
	(.126)	(.136)		
Labor market earnings ($1,000)	-5.6	-4.1	59.5	122.0
	(20.5)	(20.3)	[89.0]	[142.4]
Controls	NO	YES		
Number of firms	127	127	82	48,451
Number of individuals	223	223	120	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Initially ineligible are defined as the estimation sample to the left of the cut-off. The comparison sample of all private sector workers includes individuals who were employed by a private sector firm when aged 57–59 years (excluding bankruptcies). Earnings are measured in 2015 dollars (NOK/USD = 9).

One might argue that the employment effect can be affected by the timing of when the bankruptcy occurs, perhaps due to anticipation in the pre-period and increasing job-searching effort in the post-period. Therefore, we explore whether the RD effect is stable over time relative to the bankruptcy date. This also serves partly as a robustness check of our main result. We compute separate RD point estimates for each month m in the time span $m \in (-24, 60)$ for labor market earnings. The results are presented in Figure 5.

Figure 5: Labor supply effects over time

(a) Earnings ($1,000)



Notes: The figures show separate ITT estimates of labor market earnings (in $1,000) for each month relative to bankruptcy date. The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Point estimates are represented by the black solid line, and the dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings are measured in 2015 dollars (NOK/USD = 9).

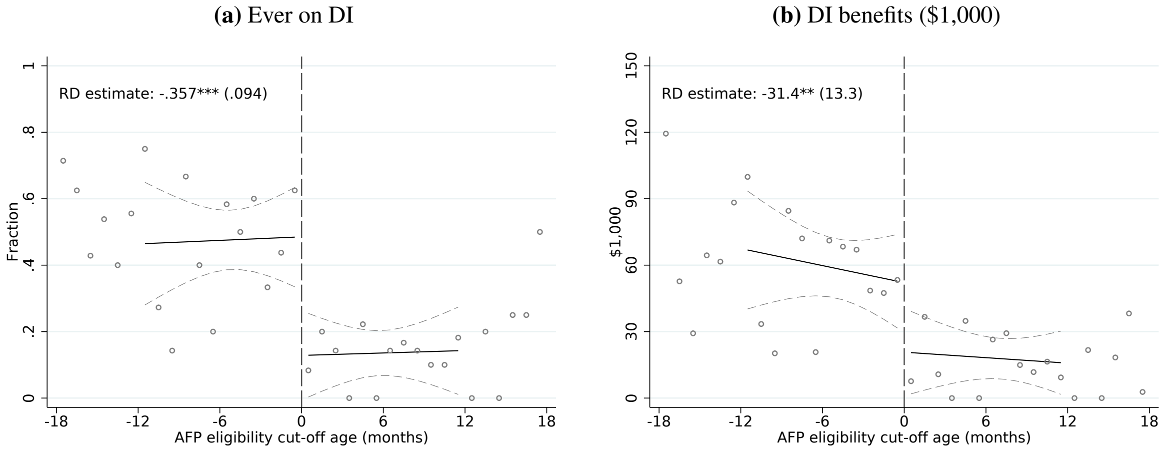
We observe that the ITT estimate on labor market earnings is very close to zero in our initial time period 24 months before bankruptcy, and then increases somewhat during the months leading up to bankruptcy. While the effect is not significant for either of these months, we observe a sharp and sizable drop in the month after the bankruptcy for which the effect remains roughly stable around zero. This might suggest that we are unable to find an effect on labor supply in the months after bankruptcy because of noise in the months prior. We therefore repeat this exercise for the sample of workers who were employed by the bankruptcy firm 12 months before and 1 month before bankruptcy as robustness checks, shown in Appendix Figure A.3. As the figures show, we are still unable to find a significant labor market earnings effect, with point estimates very stable around zero. This suggests that the additional "early leavers" in our initial estimation sample do not affect our point estimates substantially, providing further evidence of lack of labor supply responses.

5.3 Benefit substitution

As reported in the previous section, we were unable to find any effects of lost AFP eligibility on re-employment rates. A possible explanation for this could be that workers were able to offset some of the lost benefits through take-up of other social security benefits depending on eligibility. In particular, Bratsberg *et al.* (2013) showed that a large share of DI claims in Norway could be attributed to job displacements. We therefore start our analysis by investigating benefit substitution toward DI benefits. As explained in Section 2, the DI benefit in our sample period was essentially equivalent to the AFP, meaning that given the choice of AFP or DI, all else equal, workers should in principle be financially indifferent between the two benefit programs.

Disability insurance (DI) Figure 6 shows the fraction of individuals who claim DI benefits at some point between ages 62–67 (panel 6a) and the cumulative DI benefit take-up between ages 62–67 (panel 6b) around the cut-off. From panel 6a, we observe a clear discontinuity in the likelihood of claiming DI benefits depending on initial AFP eligibility. Our reduced-form RD-estimate indicates that DI claiming is about 36 percentage points lower among individuals who worked in firms where the bankruptcy occurred just after they reached the individual age threshold. As about half of those who were just initially ineligible for AFP claim DI benefits, the effect of reaching the threshold translates to a reduction in DI claiming by about 75 percent. Panel 6b shows the corresponding effect on cumulative DI benefit take-up (in $1,000). Workers who retain eligibility for AFP claim about $31,400 less DI benefits between ages 62–67, or about half of the DI benefits that individuals who do not retain eligibility receive.

Figure 6: Graphical evidence of benefit substitution towards DI between 62–67 years of age

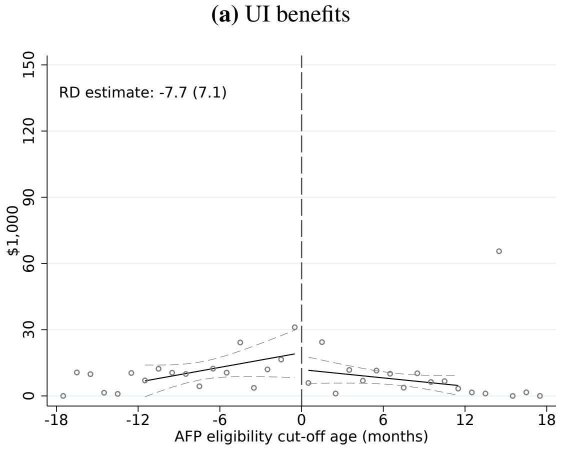


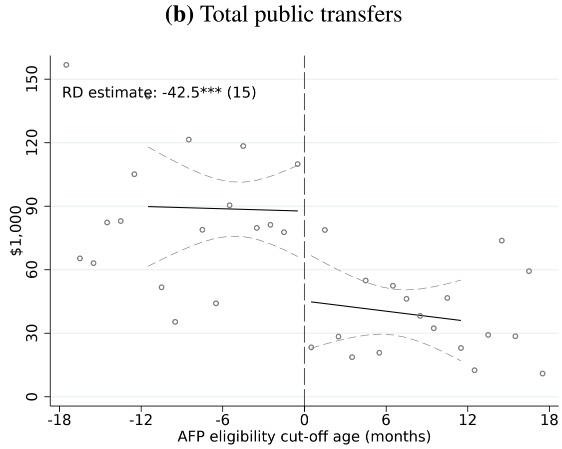
Notes: The figures show the fraction of individuals ever on DI (a) and the unrestricted means for each age-bin of cumulative DI take-up in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Benefits are measured in 2015 dollars (NOK/USD = 9).

The RD estimate for AFP benefits in panel 3b suggested that those who reached the individual eligibility age before bankruptcy date increased their take-up of AFP benefits by about $61,600. Our results thus indicate that about half the lost benefits are replaced by DI benefits. The estimates are highly significant, and we interpret this as clear evidence of program substitution toward DI benefits.

Unemployment insurance (UI) and other public transfers We now investigate whether individuals who were initially ineligible offset some of the lost AFP benefits through take-up of unemployment insurance. Additionally, we pool all public transfers (excluding AFP and old-age pensions) in order to estimate benefit substitution toward all relevant parts of the social security system. Figure 7 shows the cumulative take-up of UI benefits (panel 7a) and total public transfers (panel 7b) between ages 62–67 years (in $1,000). Although we estimate that individuals who were just initially eligible claimed less UI benefits, this effect is not significant at conventional levels. However, workers in our sample are only eligible for UI benefits for up to 2 years. As most individuals close to the cut-off are just a few months shy of turning 61 years when bankruptcy occurs, most individuals would have exhausted their UI spell before turning 62 years.[18] Panel 7b shows that initially ineligible individuals claimed significantly more non-pension public transfers. Our point estimate indicates that they claim about $42,500 more between ages 62–67, where we estimated that $31,400 is DI benefits and $7,700 is UI benefits. This suggests that a negligible $3,400 is replaced by other social security benefits.

Figure 7: Graphical evidence of unemployment insurance and total social insurance benefit take-up ($1,000) between 62–67 years of age





Notes: The figures show unrestricted means for each age-bin of cumulative UI take-up and total social insurance benefit take-up in $1,000 between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Benefits are measured in 2015 dollars (NOK/USD = 9).

Table 3 reports point estimates of AFP benefits and program substitution toward social insurance benefits. While total program substitution effects are slightly lower if we include control variables, estimates are qualitatively similar. Our estimates indicate that individuals who were just initially eligible for AFP claim about half of non-pension social security benefits compared to those who were initially ineligible. While AFP benefit take-up is $61,600 higher among workers who retain eligibility, about $42,500 are replaced with other social security benefits among those who are initially ineligible, equivalent to a replacement rate of about 69 percent. Of those, about 51 percent is DI benefits and 13 percent is UI benefits. We interpret this as substantial benefit substitution, as those who are initially ineligible due to the job displacement substantially increase take-up of other social transfers.

[18] We consider program complementarity between AFP and UI highly unlikely between ages 62–67 years as eligible individuals can claim AFP from age 62.

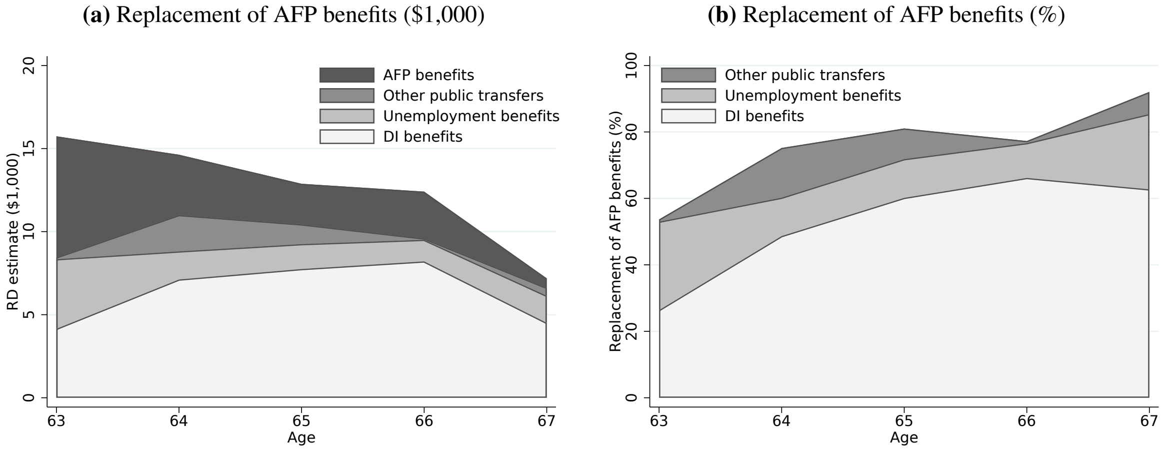
Table 3: Effect of initial AFP eligibility on cumulative social insurance benefit take-up ($1,000) between 62–67 years of age

			Mean [SD]	
Outcome:	RD estimate (ITT):		Initially ineligible	All private sector workers
AFP benefits	61.6***	57.6***	11.4	35.3
	(14.2)	(15.7)	[35.0]	[59.4]
Program substitution:				
Total public transfers	-42.5***	-36.2**	88.8	69.0
	(15.0)	(16.1)	[74.5]	[98.7]
• DI benefits	-31.4**	-24.6*	59.5	25.1
	(13.3)	(14.5)	[72.1]	[53.6]
• Unemployment benefits	-7.7	-9.5	13.2	2.7
	(7.1)	(7.2)	[24.9]	[13.8]
Controls	NO	YES		
Number of firms	127	127	82	48,451
Number of individuals	223	223	120	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Initially ineligible are defined as the estimation sample to the left of the cut-off. The comparison sample of all private sector workers includes individuals who were employed by a private sector firm when aged 57–59 years (excluding bankruptcies). Benefits are measured in 2015 dollars (NOK/USD = 9).

Effect for each age group To further investigate how those who become displaced just before the age cut-off redeem their lost benefits in terms of increased take-up of other public transfers, we run separate RD regressions for each age group. The point estimates are reported in Appendix Table A.2. Figure 8 illustrates the effects graphically. In panel 8a, the darkest area, spanning from zero, is the ITT estimate on AFP benefit take-up for each age, e.g. just reaching the individual threshold implies an increased take-up of AFP benefits by just over $15,000 at age 63. The three lighter stacked areas show how those who are initially ineligible redeem the lost benefits at each age, mainly due to lower AFP take-up among the oldest individuals while take-up of other social benefits is fairly stable across the age groups. We observe that take-up of DI benefits is by far the largest substitute, and that the degree of substitution is increasing in age. This is further illustrated in panel 8b, showing the effects on take-up of DI benefits, UI benefits and other public transfers relative to the effect on take-up of AFP benefits for each age. We observe that the increased replacement rate mainly is driven by increased replacement through take-up of DI benefits.

Figure 8: Graphical illustration of program substitution



Notes: Panel (a) illustrates the ITT effect for each outcome and each age (in $1,000). The total area is the ITT effect of AFP benefits, while the other shaded areas illustrate the ITT effect of each social insurance benefit. Panel (b) illustrates the same effects, but relative to of the ITT effect of AFP benefit take-up. The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Benefits are measured in 2015 dollars (NOK/USD = 9).

5.4 Robustness analysis

To verify the validity of our main results, we conduct a series of robustness checks. In Table 4 we present eight alternative specifications in addition to our main specification which uses a rectangular kernel and 12 months of bandwidth on each side of the cut-off. We observe that in all our robustness checks, estimates remain fairly close to our baseline specification. Take-up of AFP benefits are positive and significant for all specifications, with the point estimates being quite stable across specifications. For total public transfers (excluding pensions) and DI benefit take-up, we observe that the point estimates are negative for all our specifications and are close in magnitude. For total public transfers, all specifications are significant at the 10% level.

The first specification in Table 4 is our baseline RD estimates of the cumulative outcomes between ages 62–67. The second row adds control variables which include the pre-determined variables we use for balancing (see Appendix Table A.1) and year fixed-effects, which we observe has little impact on our main cumulative outcomes. Next, we use separate quadratic trends on each side of the discontinuity instead of separate linear trends. We observe that estimates are less precisely estimated and a magnitude larger. In specifications (iv) and (v) we check whether a local linear specification is appropriate when we deviate from the baseline choice of bandwidth. Particularly, we report estimates reducing the bandwidth by 50 percent (from 12 to 6 months) and increasing the bandwidth by 50 percent (18 months). We observe that the point estimates are very similar to the baseline specification. In Appendix Figure A.2 we extend this exercise by plotting the RD estimates with confidence intervals for each outcome. Combining the evidence from specifications (iv) and (v) with the graphical evidence in Appendix Figure A.2, we conclude that the estimates are very stable to the choice of bandwidth when we use linear trends. This suggests that linearity is a reasonable approximation to the trends around the cut-off. In specification (vi) we use a triangular kernel (rather than rectangular kernel) which has negligible impact on our estimates. Specifications (vii) and (viii) change the pre-determination of employment status in bankruptcy firms from 24 months before bankruptcy to 12 months before and 1 month before, respectively. Reassuringly, the point estimates are quite similar to our main specification although estimates in the latter specification are less precise due to the smaller sample size. Finally, specification (ix) includes bankruptcies where at least 1/3 of (all) employees switched to the same firm which we deemed as "spurious" bankruptcies. As expected, the estimated effects are smaller in magnitude when we include these firms as a larger share of workers did not experience a job displacement but were rather collectively moved to a new firm.

Table 4: Specification checks

				Program substitution:			
		AFP benefits	Labor market earnings	Total public transfers	DI benefits	Unemployment benefits	Obs <Firms>
Column:		(1)	(2)	(3)	(4)	(5)	(6)
i:	Baseline RD estimate	61.6***	-5.6	-42.5***	-31.4**	-7.7	223
		(14.2)	(20.5)	(15.0)	(13.3)	(7.1)	<127>
ii:	With controls	57.6***	-4.1	-36.2**	-24.6*	-9.5	223
		(15.7)	(20.3)	(16.1)	(14.5)	(7.2)	<127>
iii:	Quadratic trends	73.5***	30.2	-67.0***	-40.4**	-12.6	223
		(19.2)	(34.8)	(23.0)	(20.2)	(11.8)	<127>
iv:	Bandwidth:	75.9***	19.3	-43.6**	-26.9	-9.9	115
	50% lower	(20.3)	(33.2)	(22.2)	(18.6)	(11.0)	<70>
v:	Bandwidth:	64.4***	-8.8	-42.5***	-32.3***	-9.0	305
	50% higher	(12.6)	(17.5)	(13.7)	(12.3)	(5.9)	<160>
vi:	Triangular kernel	66.3***	8.4	-52.0***	-35.0**	-9.6	223
		(14.4)	(24.5)	(16.4)	(13.8)	(8.6)	<127>
vii:	Workers 12 months	60.6***	.4	-44.9***	-34.0**	-5.7	213
	pre-bankruptcy	(14.9)	(21.7)	(17.0)	(15.1)	(7.1)	<124>
viii:	Workers 1 month	62.9***	.4	-37.0*	-27.9	-5.6	163
	pre-bankruptcy	(16.2)	(27.5)	(20.0)	(17.5)	(8.9)	<96>
ix:	With "spurious"	49.0***	-2.3	-26.5*	-18.4	-3.0	290
	bankruptcies	(13.2)	(22.5)	(14.5)	(12.2)	(6.6)	<161>

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local RD regressions for each outcome (in $1,000) and each respective specification. All specifications use linear separate linear trends except specification (iii) which uses separate quadratic trends. Main specification (i) uses a rectangular kernel and 12 months of bandwidth. Controls in specification (ii) include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Specification (vii) and (viii) includes workers who worked in bankruptcy firm 12 and 1 month respectively before the bankruptcy date (all other specifications include individuals who worked in firm 24 months before bankruptcy). Specification (ix) also includes bankruptcies where at least 1/3 of (all) employees switched to the same firm. The sample consists of individuals employed by a firm with AFP affiliation 24, 12 or 1 month(s) (depending on specification) before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

We also perform a placebo test by using private sector bankruptcy firms *without* AFP coverage in an otherwise similar setup to our baseline sample. As the "cut-off" for these workers does not involve the loss (or gain) of early retirement eligibility, our main outcomes should have the same distribution just before and just after the hypothetical cut-off. The estimated effects of our cumulative outcomes are relegated to Appendix Table A.3 and shown graphically in Appendix Figure A.5. We are unable to reject the null of no difference between workers on each side of the cut-off for any of our main outcomes. There is, as expected, a close-to-zero effect on AFP benefit take-up, as the only way for these individuals to become eligible for AFP benefits is to switch workplace to a firm with AFP coverage and acquire at least three years of tenure. While the point estimate for labor market earnings is positive, and the point estimates for public transfers and DI benefits are negative, the estimates are roughly within one standard error.

5.5 Heterogeneity

As workers in our estimation sample differ somewhat in characteristics compared to the average private sector worker, we further investigate the driving forces behind the main responses. Particularly, Table 1 revealed that workers in our sample are typically male workers in the manufacturing sector. To understand to which extent our results have external validity, we therefore explore heterogeneous effects. Workers' wages and education may also be important; workers with high wages are likely eligible for a higher AFP benefit as the benefit is linked to past earnings, which may result in loss of eligibility for AFP being a larger shock to individuals with higher wages. However, workers with high wages may also have better outside options in the labor market than workers with low wages, and may have lower search costs when unemployed.[19] We therefore expect that workers with higher pre-bankruptcy earnings have higher re-employment rates, and possibly lower program substitution rates.

To determine how the pattern of labor market adaptation and take-up of social benefits differ across worker groups, we use the same initial estimation sample and empirical strategy on subsets of workers. In Table 5 we report estimates of our main cumulative outcomes between ages 62–67 corresponding to differences in gender, pre-bankruptcy earnings, educational attainment and industry.[20]

The estimated coefficients for men indicate that they exhibit similar properties as the full estimation sample. For women, the point estimates are smaller, but also more imprecise mainly due to the small sample size. While we lack precision to provide a definitive answer to whether there are differences between genders, the estimates suggest that men are more likely to respond to the incentive to claim AFP benefits and reduce take-up of other social benefits, while women to a larger extent claim other social security benefits regardless of having the option to retire early.[21]

To explore heterogeneous effects in pre-bankruptcy earnings, we split our sample on earnings (24 months) prior to bankruptcy. As expected, compared to high earnings workers, the effect on AFP benefit take-up is smaller for workers with low earnings (smaller than or equal to the median). This difference is likely somewhat mechanical as low earnings workers have lower accrual of AFP on average. However, we observe that low earnings workers replace almost the entire loss of AFP benefits with other social security benefits, while high earnings workers replace a significantly lower share. In fact, the estimated coefficients for high earnings workers on our social security outcomes are not significantly different from zero at conventional levels. While this suggest that high earnings workers may have better outside options and respond to the labor supply incentives, we observe that the estimated coefficients on labor market earnings, although imprecise, are practically indistinguishable between the two groups.

[19] Similarly, education may be correlated with better outside options, as education is highly correlated with earnings.

[20] For the latter subgroup, we explore manufacturing specifically, as this is the by far largest subgroup of workers within the private sector AFP workers.

[21] When exploring gender differences, we would ideally also want to explore spousal spillover effects. We estimated the effect on spousal outcomes and found no effects on employment or take-up of any social security benefits for the spouse. We emphasize that this should be interpreted with caution due to our small sample size, although the point estimates are close to zero.

Table 5: Subsample analysis of labor market earnings and social insurance benefit take-up ($1,000) between 62–67 years of age

	AFP benefits	Labor market earnings	Program substitution: Total public transfers	DI benefits	Unemployment benefits	Obs <Firms>
Column:	(1)	(2)	(3)	(4)	(5)	(6)
Full sample	61.6***	-5.6	-42.5***	-31.4**	-7.7	223
	(14.2)	(20.5)	(15.0)	(13.3)	(7.1)	<127>
	[11.4]	[59.6]	[88.8]	[59.5]	[13.2]	
Males	70.7***	-8.2	-44.9***	-39.1**	-3.1	173
	(14.8)	(24.8)	(17.4)	(16.2)	(5.7)	<101>
	[11.2]	[66.6]	[89.9]	[62.7]	[11.2]	
Females	29.8	-12.5	-30.0	-7.9	-17.4	50
	(37.7)	(29.3)	(32.8)	(25.3)	(20.5)	<42>
	[12.1]	[36.4]	[85.1]	[49.2]	[19.5]	
High earnings	75.0***	-3.5	-32.7	-28.9	-6.5	108
	(23.9)	(34.0)	(24.8)	(21.2)	(10.0)	<65>
	[13.8]	[74.2]	[91.4]	[57.1]	[14.4]	
Low earnings	51.1***	-4.9	-50.5**	-32.7*	-9.2	115
	(18.3)	(22.6)	(19.9)	(18.3)	(9.2)	<87>
	[9.2]	[45.8]	[86.3]	[61.8]	[12.1]	
High education	21.9	-7.5	-5.2	-5.1	-2.1	75
	(27.7)	(41.4)	(34.8)	(27.8)	(6.1)	<50>
	[18.7]	[89.7]	[68.9]	[42.6]	[7.9]	
Low education	81.8***	.8	-63.3***	-46.3***	-11.5	148
	(16.5)	(21.1)	(17.3)	(16.1)	(9.3)	<99>
	[7.4]	[42.7]	[99.9]	[69.0]	[16.1]	
Manufacturing	47.8***	18.7	-46.9**	-49.4***	4.1	149
	(16.6)	(22.5)	(19.3)	(17.1)	(6.9)	<72>
	[14.3]	[58.5]	[93.7]	[63.9]	[11.9]	
Other industries	96.6***	-53.1	-42.8*	-6.6	-29.7**	74
	(25.7)	(37.9)	(24.7)	(19.0)	(12.0)	<55>
	[6.4]	[61.4]	[80.0]	[51.6]	[15.5]	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity. Independent means of initially ineligible (the sample to the left of cut-off) in brackets.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000) and each subgroup. High earnings are defined as larger than median 24 months before bankruptcy date, and low earnings otherwise. High education is defined as completed high school or more, and low education otherwise. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

When we split our sample on educational attainment (high education is defined as completed high school and low education otherwise), we find a quite similar pattern as when we split our sample on earnings prior to bankruptcy, although with one notable exception; the point estimate on AFP benefits is large and highly significant for workers with low education, but rather low and insignificant for workers with high education. The point estimates on our social security outcomes are significantly larger for low-education workers and gives relatively clear evidence of responses being driven by low education workers.

In our estimation sample, around 68 percent of workers are employed in the manufacturing industry compared to 30 percent of all private sector firms. To investigate the external validity of our findings, we therefore do separate estimations for workers in the manufacturing industry and workers who were employed in other industries. We observe that point estimates on AFP benefits are smaller for workers in the manufacturing industry. However, this is not because of differences in wages; in fact, workers in the manufacturing have comparable earnings to workers in other industries prior to bankruptcy. While the point estimates of total public transfers are similar between the two subgroups, manufacturing workers replace a much larger share of the lost AFP benefits with other social security benefits compared to other workers. In fact, the point estimates suggest that manufacturing workers replace the entire lost AFP benefits with DI benefits, suggesting that workers in more physically demanding jobs are more inclined to be eligible and possibly apply for DI benefits. There is no evidence for such replacement for workers in other industries. In fact, there is clear evidence of workers in other industries replacing some of the lost AFP benefits with unemployment benefits, with a coefficient significant at the 5% level. Interestingly, the point estimate of labor market earnings is negative and relatively large for workers in non-manufacturing industries compared to manufacturing workers. While not significant at conventional levels, it may seem that the lack of a labor supply response for our main estimation sample could be driven by manufacturing workers. A possible explanation for this could be because of low local labor demand, and in particular for workers with specific occupational skills, as a relatively large share of the manufacturing firms in our sample were relatively large firms located in small towns.

6 Instrumental variable estimates

While our main findings show that being initially eligible for AFP based on employment status 24 months prior to bankruptcy affects AFP claiming and take-up of social security benefits, these findings may underestimate the true effects of being eligible for AFP as some initially eligible individuals may leave the firm early and not satisfy the eligibility criteria, and some individuals who were initially ineligible may regain eligibility if re-employed in a different firm covered by the AFP scheme. In this section, we therefore use the individual eligibility age as an instrument for AFP eligibility in an instrumental variables (IV) setup in an attempt to estimate the true effect of optional early retirement. This approach yields the local average treatment effect (LATE), that is the average effect of having the option to retire early for compliers in our sample (Imbens & Angrist, 1994). In our setting, the compliers are workers who become eligible for early retirement because their age is above the eligibility cut-off but would not have become eligible otherwise. In our alternative fuzzy RD design, the empirical model can be summarized by the following two equations:

$$E_i = \alpha_0 + \alpha_1 \mathbb{Z}_{a_i \geq 0} + f(a_i) + \delta X_{it} + \varepsilon_{it} \tag{6}$$

$$y_{it} = \beta_0 + \beta_1 E_i + f(a_i) + \delta X_{it} + \varepsilon_{it} \tag{7}$$

where E_i takes the value one if individual i is eligible for AFP and zero otherwise, X_{it} is a set of covariates and y_{it} is the outcome of interest for individual i at time t, ε_{it} is the error term and f is an unknown functional form of the assignment variable. The indicator variable $\mathbb{Z}_{a_i \geq 0}$ is the instrumental variable, taking the values:

$$\mathbb{Z}_{a_i \geq 0} = \begin{cases} 0 & \text{if} \quad a_i < 0 \\ 1 & \text{if} \quad a_i \geq 0 \end{cases} \tag{8}$$

where a_i is defined as in Equation (1), meaning that if individuals' age at the bankruptcy date is above the threshold, the instrument takes the value one, and zero otherwise. It is crucial that $\mathbb{Z}$ is uncorrelated with potential measurement error in E. While we are able to construct a fairly accurate measure for eligibility by determining who is eligible based on the criteria outlined in Section 2.1, we cannot observe eligibility directly. Because of this, it is possible that our treatment variable is measured with some errors.[22] While measurement error in the treatment variable in an IV setting creates a bias in the estimator (see e.g. Lewbel, 2007; Jiang & Ding, 2020; Yanagi, 2019), Ura (2018) and Yanagi (2019) showed that under the assumption that the instrumental variable is uncorrelated with the measurement error in the treatment variable (i.e. the probability of misclassification of treatment), the Wald estimator gives an upper bound estimate in absolute value of the true coefficient.

Additionally, it is not clear how to define the treatment in our setting as individuals' eligibility status could change depending on employment status and the various other criteria for AFP. Therefore, some non-treated or treated individuals could be partially treated. We decide to define our treatment as eligible for AFP at some point between ages 62–67 years as most partially treated individuals will regain eligibility shortly after the earliest point of withdrawal (e.g. at ages 62 or 63). In practice, this means that our estimates will serve as upper bound estimates as some individuals we define as treated will be partially treated. As potential measurement errors in our treatment variable will also contribute to overestimate the true effects, we therefore emphasize that the IV estimates should be interpreted as upper bound estimates of the effect of access to early retirement. However, we argue that the IV estimates are useful for scaling of our main findings and interpretation of the true effect of AFP eligibility on our outcomes.

A key identifying assumption for the IV to be valid is the exclusion restriction, i.e. the instrument must be conditionally independent of potential outcomes. We argue that the exclusion restriction holds in our case as just reaching a certain age in itself does not affect employment or take-up of other social security benefits, but only because age affects eligibility. As a further argument for this claim, our placebo estimates of non-AFP workers reported in Appendix Table A.3 indicate that outcomes of ineligible individuals are indeed similar around the age-threshold. Another key identifying assumption is monotonicity in responses. We consider "defiers" highly unlikely in our setting as this would imply that some individuals become eligible because age is just below the threshold but would not have become eligible otherwise. Finally, the instrument must be relevant, i.e. just reaching the individual age-threshold must affect eligibility. We verify this when summarizing our results.

The results of our fuzzy RD model are presented in Table 6. For comparison with our main estimates, we also include the ITT estimates from the reduced form RD model. We emphasize that our instrument has a high predictive power of the treatment variable. Our first-stage estimate shows that the probability of being eligible for AFP is among 70 percentage points higher among those who just reached the

[22] Out of the 199 individuals we classified as ineligible following the standard criteria, 4 individuals in our sample or around 2 percent were observed with actual take-up of AFP. Unfortunately, we are unable to provide a measure of the number of individuals we classify as eligible whose true status are in fact ineligible as we cannot distinguish these individuals from never-takers of AFP.

individual cut-off age at the firm bankruptcy date. We estimate that compliers increase AFP take-up with $87,900 and decreases take-up of other social benefits by $60,600, where $44,800 of this is due to decreased take-up of DI when becoming eligible for AFP.

Table 6: IV estimates of cumulative outcomes ($1,000)

Treatment variable:	First stage:				Mean [SD]	
Eligible for AFP	.70***	.67***				
	(.08)	(.09)				
Outcome:	IV estimate (2SLS):		Reduced form (ITT):		Initially ineligible	All private sector workers
AFP benefits	87.9***	85.5***	61.6***	57.6***	11.4	35.3
	(19.5)	(21.0)	(14.2)	(15.7)	[35.0]	[59.4]
Labor market earnings	-8.0	-6.1	-5.6	-4.1	59.5	122.0
	(29.0)	(28.4)	(20.5)	(20.3)	[89.0]	[142.4]
Total public transfers	-60.6***	-53.7**	-42.5***	-36.2**	88.8	69.0
	(21.2)	(21.6)	(15.0)	(16.1)	[74.5]	[98.7]
• DI benefits	-44.8**	-36.5*	-31.4**	-24.6*	59.5	25.1
	(19.5)	(20.4)	(13.3)	(14.5)	[72.1]	[53.6]
• Unemployment benefits	-10.9	-14.0	-7.7	-9.5	13.2	2.7
	(9.7)	(9.4)	(7.1)	(7.2)	[24.9]	[13.8]
Controls	NO	YES	NO	YES		
Number of firms	127	127	127	127	82	48,451
Number of individuals	223	223	223	223	120	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows the 2SLS estimates of fuzzy RD regressions using AFP eligibility as the treatment variable, and the corresponding reduced form estimates. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). Controls in the alternative specifications include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

7 Implications

In this section, we assess the implications of our findings for policy and welfare for the displaced workers in our sample. While access to an early retirement program provides better insurance for displaced workers, it could also increase public expenditures through increased benefit payments and decreased tax revenues. However, as we have shown, decreased benefit payments of other social security benefits could offset some of the increased costs. These trade-offs are particularly important in assessing the desirability of the program.

To assess how access to early retirement affects public finances, we estimate our RD model on net public expenditures as the outcome variable, defined as net benefit payments from (all) social security benefits net of payroll taxes from earnings (including income from self-employment). As a rough measure of how access to early retirement affects workers' welfare, we consider disposable income as an outcome variable, defined as total income from social security and earnings net of taxes. Finally, we investigate savings as our third outcome variable defined as the annual change in wealth. To ease interpretation, we do estimations on an annual basis when individuals are between 62 and 67 years of age.[23]

In Table 7, we report IV estimates from our fuzzy RD model as well as ITT estimates from our main reduced form model. As we report estimates at the annual level, we consider individuals' eligibility status for AFP also at the annual level, and cluster standard errors at the individual and firm level. Our estimates indicate that access to the AFP program had only a small impact on public finances. This is not surprising given our previous findings, where we did not find evidence of an effect on labor supply, but relatively large substitution effects onto other social security programs. At the 95% confidence level, our IV estimate suggests that the annual increase in public expenditures is at most \$16,400 for compliers in our sample. Our estimates also indicate that access to the AFP program had little impact on the average welfare for individuals. Due to lack of significance, we cannot conclude that access to early retirement increased average disposable income for individuals. However, we can rule out a large decrease in the average welfare for ineligible individuals. At the 95% confidence level, our IV estimate suggest that the annual effect on disposable income is at most \$10,300 for compliers in our sample. We are also unable to conclude that access to early retirement had an effect on savings. Note that average savings are positive among initially ineligible. Taken together, this suggests that most ineligible individuals had some source of income.

Table 7: Annual financial costs and benefits (\$1,000)

Treatment variable:	First stage:				Mean [SD]	
Eligible for AFP	.78***	.74***				
	(.08)	(.09)				
Outcome:	IV estimate (2SLS):		Reduced form (ITT):		Initially ineligible	All private sector workers
Net public expenditures	7.2	7.1	5.6	5.3	14.8	10.1
	(4.7)	(4.8)	(3.6)	(3.6)	[18.9]	[30.7]
Disposable income	3.8	4.2	2.9	3.1	31.7	40.5
	(3.3)	(2.6)	(2.6)	(1.9)	[11.6]	[23.4]
Savings	1.6	1.7	1.2	1.3	.9	3.2
	(3.1)	(3.1)	(2.5)	(2.3)	[33.5]	[43.0]
Controls	NO	YES	NO	YES		
Number of firms	124	124	124	124	79	48,644
Number of individuals	216	216	216	216	116	138,644
Number of observations	1,224	1,224	1,224	1,224	667	798,228

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the individual and firm level and are robust to heteroskedasticity.
Notes: The table shows the 2SLS estimates of fuzzy RD regressions using AFP eligibility as the treatment variable, and the corresponding reduced form estimates. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in \$1,000). Controls in the alternative specifications include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Net public expenditures are defined as net benefit payments from all social security programs subtracting payroll taxes from earnings. Disposable income is defined as benefit payments and earnings net of taxes. Savings are defined as the change in annual wealth. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Variables are measured in 2015 dollars (NOK/USD = 9).

[23]To compare these estimates to our main cumulative outcomes, these estimates should therefore be multiplied by 6, as years between ages 62–67 include 6 calendar years.

To further investigate this claim, we follow the standard framework of Imbens & Rubin (1997) and estimate marginal distributions of disposable income under different treatment statuses for compliers. If a larger share of individuals are significantly worse off, this might be of particular interest for policy-makers. More specifically, we use eligibility age above cut-off $(\mathbb{Z})$ as an instrument for AFP eligibility (E) in a standard Imbens & Rubin (1997) framework. The marginal distributions of potential outcomes for compliers g_e where e is treatment status are defined as:

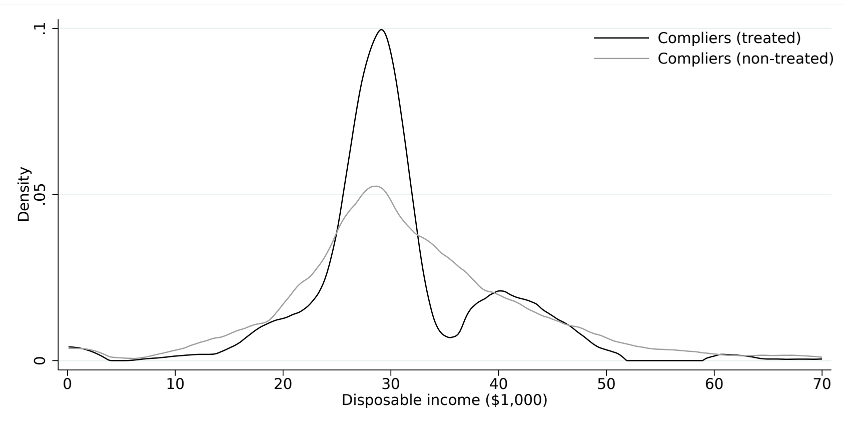
$$g_0(y) = f_{00}(y) \cdot (p_c + p_c)/p_c - f_{10}(y) \cdot p_n/p_c \tag{9}$$

$$g_1(y) = f_{11}(y) \cdot (p_c + p_c)/p_c - f_{01}(y) \cdot p_a/p_c \tag{10}$$

where f_{ze} is the distribution of disposable income for individuals with z being equal to 1 if eligibility age is above cut-off and 0 otherwise and treatment status $e = 0,1$. p_a is the proportion of "always-takers", p_n is the proportion of "never-takers" and p_c is the proportion of compliers. We estimate f using an epanechnikov kernel with optimal bandwidth.

Figure 9 shows the estimated distributions of potential disposable income for compliers in our sample, that is, the individuals who become eligible for AFP because their age is above the eligibility cut-off but would not have become eligible otherwise. Evidently, the disposable income of eligible compliers is more concentrated around the mean with a rather small dispersion. In contrast, the dispersion is higher among ineligible compliers, with a slight tendency of a fatter right-tail, meaning that a larger proportion have higher disposable income. Even though there is evidence of a slightly larger proportion of ineligible compliers having low disposable income, the difference in the lower part of the distribution is almost indistinguishable. This suggests that a very low share of ineligible individuals are significantly worse off because of failing to qualify for early retirement, with most individuals getting some source of income either through participation in the labor market or receiving some type of social security benefit.

Figure 9: Potential outcomes for compliers: Disposable income ($1,000)



Notes: The figure shows distributions of potential disposable income for compliers as defined by Imbens & Rubin (1997) (see text for details). Densities are estimated using an epanechnikov kernel with optimal bandwidth of 1.74. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Disposable income is defined as earnings and benefits excluding taxes and is measured in 2015 dollars (NOK/USD = 9).

8 Conclusion

In this paper, we have asked how the loss of eligibility for early retirement benefits among displaced workers affects re-employment rates and spillover onto other social security programs. We have used detailed register data with information on exact dates of firm bankruptcies which allowed us to causally estimate effects of individual eligibility for early retirement provision.

Using a regression discontinuity research design which compares workers where some end up “reaching the threshold” for eligibility before a firm bankruptcy while some do not, we have been unable to find that early retirement provision induces unintended adverse effects on re-employment. Furthermore, our findings suggest that the loss of early retirement eligibility induces substantial excess take-up of DI benefits among displaced workers. Tight eligibility criteria therefore may induce workers to excessively apply for other social security benefits.

We emphasize that our findings are mainly driven by male, low educated workers in the manufacturing sector. While we do not find significant effects for female workers or high educated workers, we find an offsetting effect on UI benefit take-up among workers in non-manufacturing industries, but no effect on DI for these workers. Moreover, we take several steps to ensure the validity of our findings and show that our main conclusions do not change depending on specifications of the RD design. Reassuringly, our results are not sensitive to the choice of when to pre-determine employment in the firm or the choice of bandwidth.

While access to an early retirement program provides better insurance for displaced workers, it could also increase public expenditures through increased benefit payments and decreased tax revenues. We showed that the early retirement program did not significantly increase public expenditures, as ineligible workers did not increase their labor supply but rather claimed other social security benefits. Therefore, we conclude that provision of early retirement for displaced elderly workers is desirable for policymakers, and that too tight eligibility criteria might be harmful as it induces considerable program substitution.

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Appendix A Additional Tables and Figures

Table A.1: Smoothness of predetermined covariates

	Main est. sample: AFP workers			**Placebo sample: Non-AFP workers**		
Dependent variable:	*coeff.*	*std. error*	*p-value*	*coeff.*	*std. error*	*p-value*
Female	-.093	(.109)	.395	.050	(.087)	.568
Married	.015	(.113)	.893	-.068	(.090)	.448
Years of education	-.236	(.435)	.588	.324	(.475)	.496
Tenure	-.85	(2.45)	.728	.58	(1.45)	.688
Number of employees	17	(36)	.640	-1.51	(2.89)	.600
Monthly earnings ($1,000)	-.155	(.530)	.771	.387	(.406)	.341
Manufacturing	.073	(.123)	.554	-.177**	(.084)	.036
Full time employment	.077	(.064)	.228	-.121**	(.053)	.023
Local DI rate	.014**	(.007)	.037	.005	(.005)	.307
Local unemp. rate	-.001	(.002)	.687	.000	(.002)	.871
Share senior workers	.026	(.029)	.383	.028	(.042)	.505
Wealth ($1,000)	-27	(24)	.263	.2	(20.1)	.993
Sickness benefits	-.043	(.096)	.655	.002	(.065)	.975
Joint test			.402			.228
Number of individuals (firms)	223	(127)		417	(372)	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each pre-determined covariate. Each covariate is measured 24 months before bankruptcy date for each employee. Local DI rate and unemployment rates are measured at the municipality level. The share of senior workers is defined as the share of (all) coworkers above 57 years (excluding self). The main estimation sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The placebo sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria. Both samples include bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

Table A.2: Effect of initial AFP eligibility on labor market earnings and social insurance benefit take-up ($1,000) by age

	AFP benefits	Labor market earnings	Program substitution: Total public transfers	DI benefits	Unemployment benefits	Obs <Firms>
Column:	(1)	(2)	(3)	(4)	(5)	(6)
Total effect	61.6***	-5.6	-42.5***	-31.4**	-7.7	223
62-67 years	(14.2)	(20.5)	(15.0)	(13.3)	(7.1)	<127>
Effect by age:						
62 years	7.2***	-2.7	-1.8	-3.7	1.5	216
	(2.0)	(8.3)	(3.7)	(2.9)	(3.2)	<124>
63 years	15.9***	-1.2	-8.4**	-4.1	-4.2**	214
	(3.2)	(7.7)	(3.5)	(3.1)	(1.9)	<124>
64 years	14.3***	-1.9	-10.8***	-7.0**	-1.7	212
	(3.3)	(5.3)	(3.3)	(3.0)	(1.5)	<123>
65 years	13.3***	1.0	-10.6***	-7.9***	-1.5	211
	(3.0)	(4.3)	(3.3)	(3.0)	(1.2)	<123>
66 years	12.4***	1.2	-9.6***	-8.2***	-1.3	208
	(3.0)	(4.3)	(3.1)	(2.8)	(1.1)	<122>
67 years	7.2***	-3.7	-6.6***	-4.5**	-1.6*	163
	(2.4)	(3.5)	(2.3)	(2.1)	(1.0)	<91>

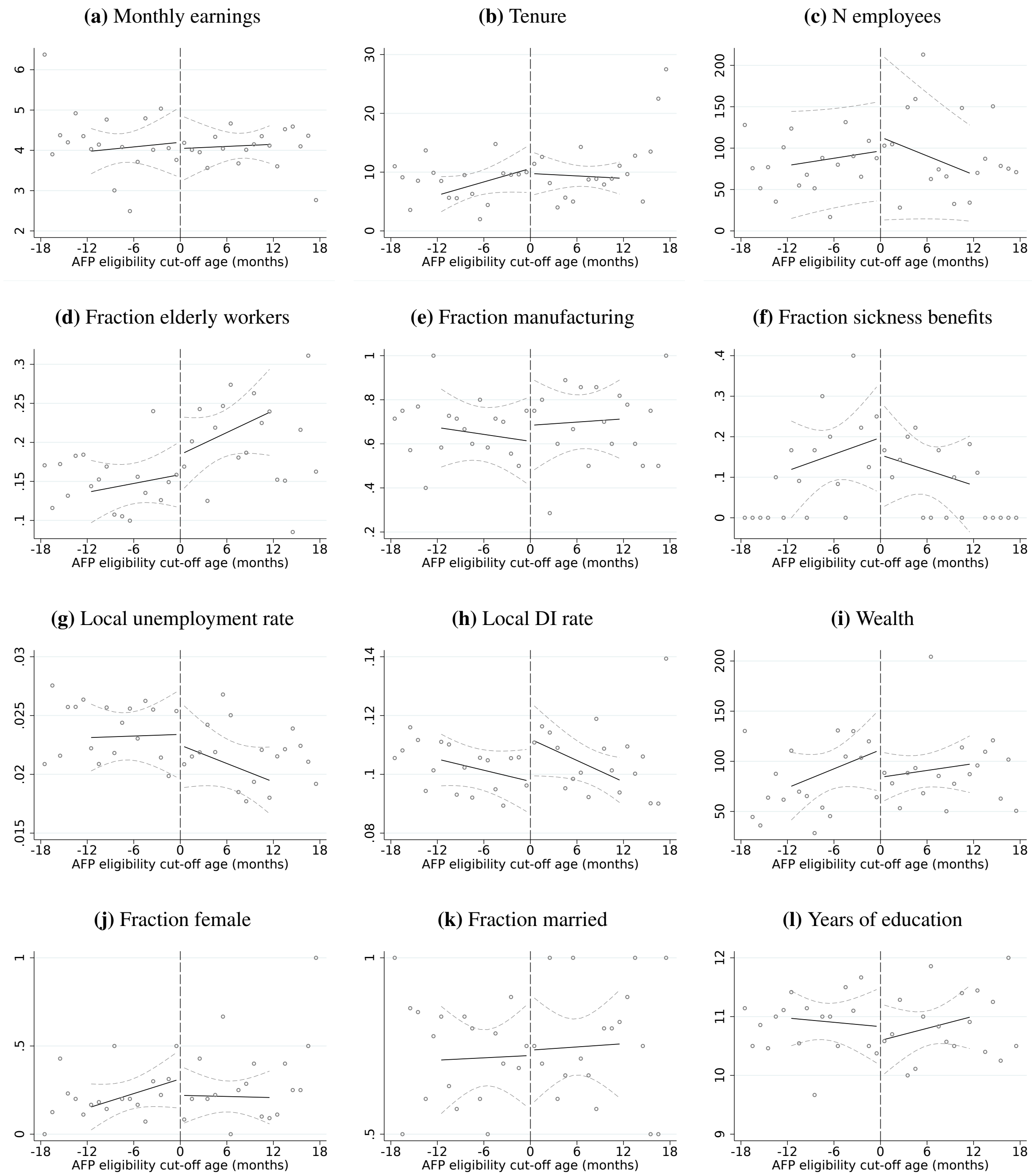
*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

Table A.3: Placebo estimates of cumulative outcomes (in $1,000): Non-AFP bankruptcies

			Mean [SD]	
Outcome:	RD estimate (ITT):		Initially ineligible	All private sector workers
AFP benefits	.8	1.8	4.4	35.3
	(2.1)	(2.1)	[23.5]	[59.4]
Labor market earnings	20.0	19.3	79.0	122.0
	(23.5)	(21.4)	[104.0]	[142.4]
Program substitution:				
Total public transfers	-6.1	-3.3	76.7	69.0
	(16.2)	(15.7)	[83.2]	[98.7]
• DI benefits	-15.2	-11.0	40.0	25.1
	(13.7)	(12.8)	[64.5]	[53.6]
• Unemployment benefits	3.1	5.6	10.9	2.7
	(5.8)	(6.0)	[26.8]	[13.8]
Controls	NO	YES		
Number of firms	372	372	201	48,451
Number of individuals	417	417	221	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Placebo sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Initially ineligible are defined as the sample to the left of the cut-off. The comparison sample of all private sector workers includes individuals who were employed by a private sector firm when aged 57–59 years (excluding bankruptcies). Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

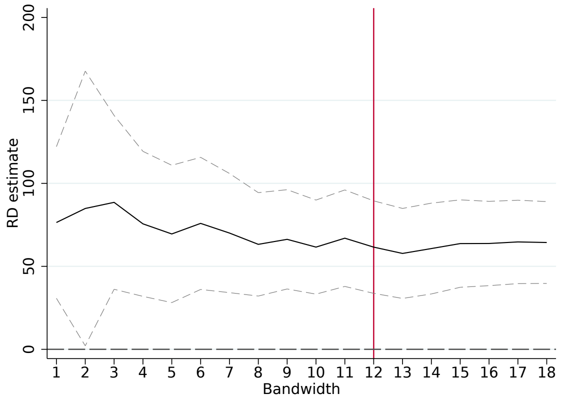
Figure A.1: Characteristics around cut-off



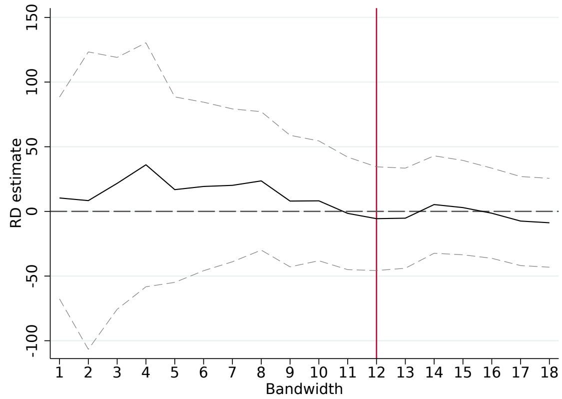
Notes: The figures show the unconditional means of each pre-determined covariate for each monthly age-bin relative to cut-off. Each covariate is measured 24 months before bankruptcy date. The black solid lines illustrate results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. Local DI rate and unemployment rates are measured at the municipality level. The share of senior workers are defined as the share of (all) coworkers above 57 years (excluding self). The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

Figure A.2: RD estimates and bandwidth selection: Cumulative outcomes

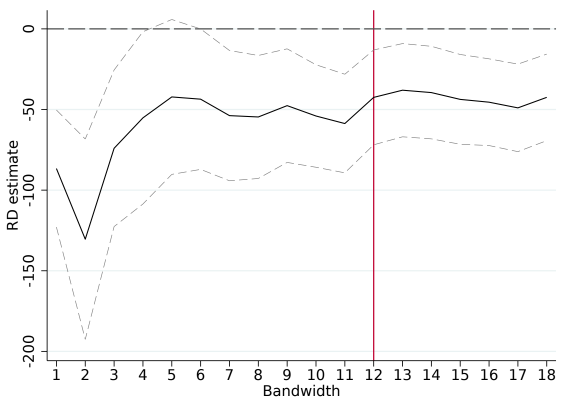
(a) AFP benefit take-up



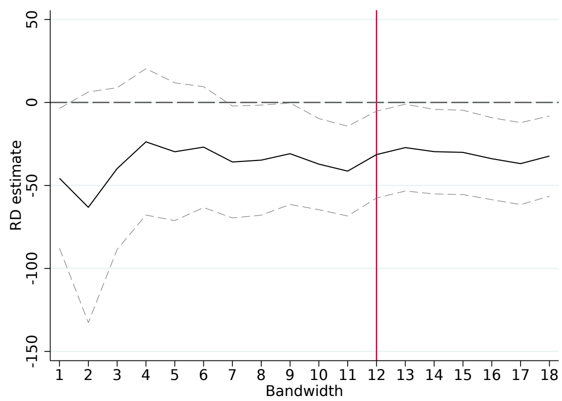
(b) Labor market earnings



(c) Public transfers



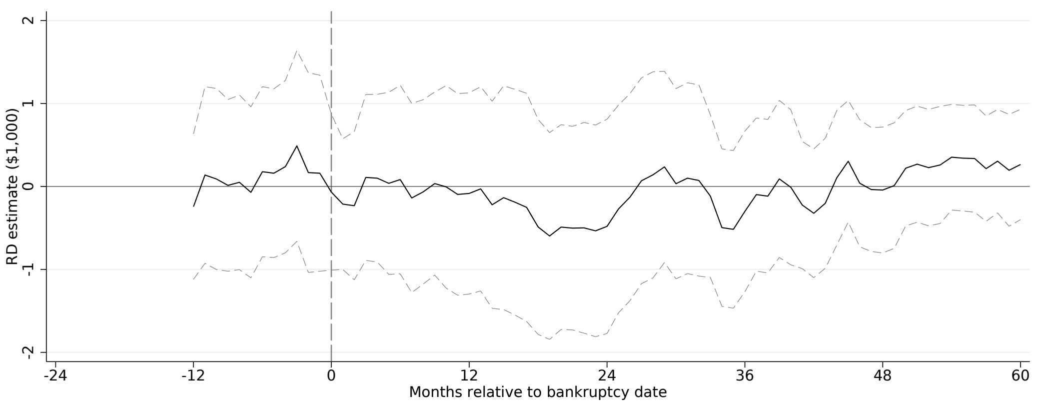
(d) DI benefits



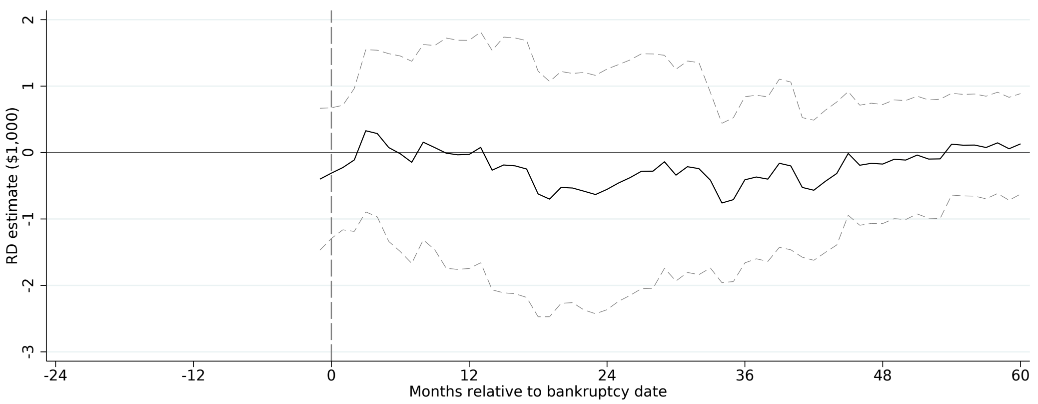
Notes: The figures illustrate the estimated ITT effect for each outcome (in $1,000) for each choice of bandwidth (indicated on the horizontal axis). The ITT effects are estimated by RD regressions using a local linear regression and a rectangular kernel on each side of the cut-off. The red vertical line represents the baseline bandwidth choice of 12 months. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

Figure A.3: Labor market earnings effects over time (in $1,000) for alternative sample of workers

(a) Workers employed 12 months before bankruptcy

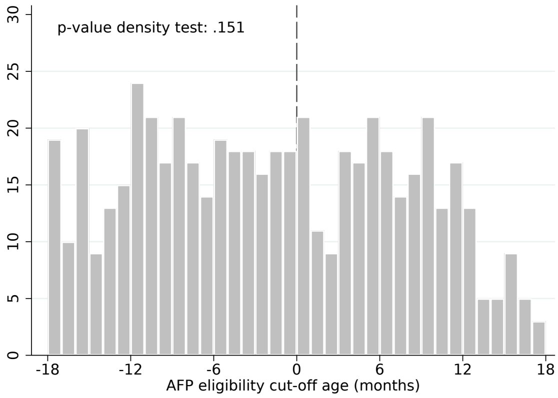


(b) Workers employed 1 month before bankruptcy



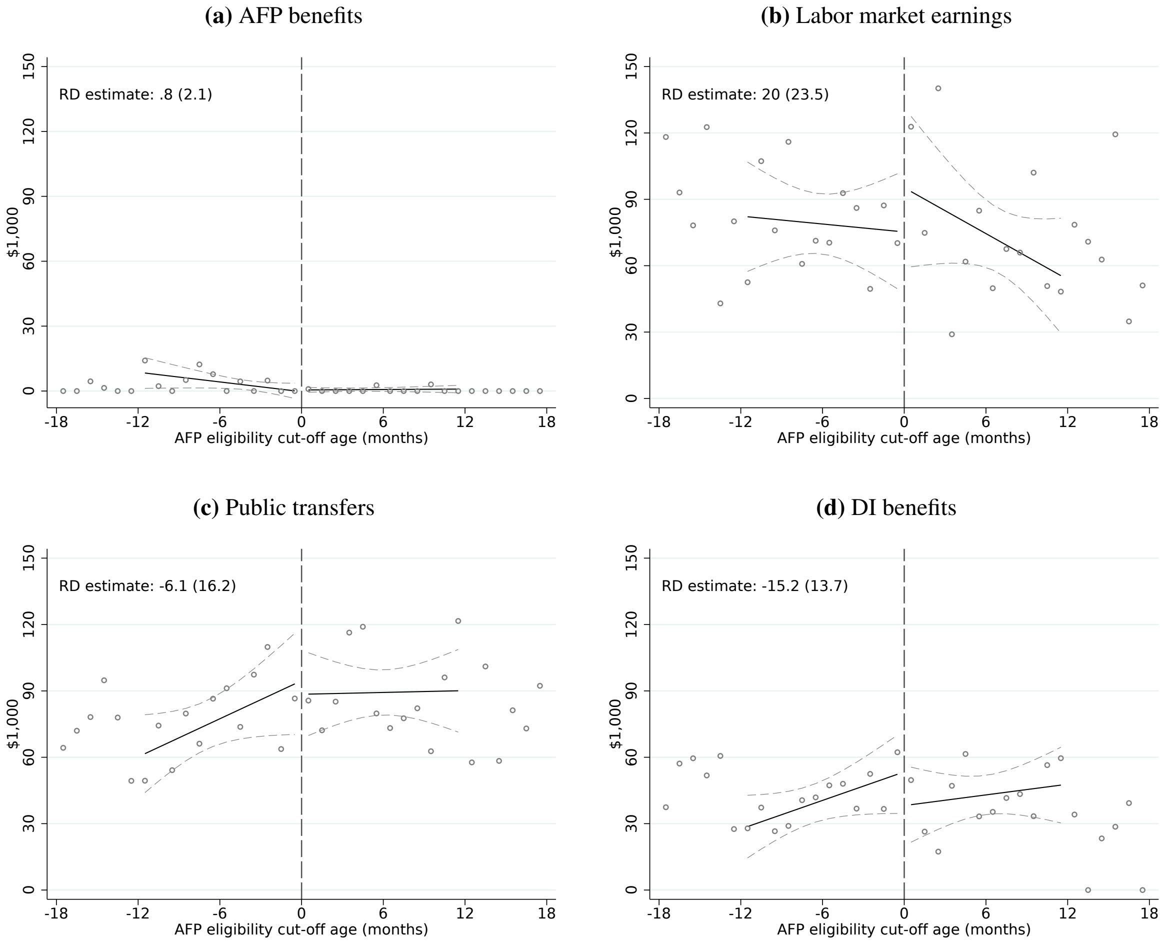
Notes: The figures show separate ITT estimates of labor market earnings (in $1,000) for each month relative to bankruptcy date for the sample of workers employed 12 months before bankruptcy (top graph) and 1 month before bankruptcy (bottom graph). The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Point estimates are represented by the black solid line, and the dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings are measured in 2015 dollars (NOK/USD = 9).

Figure A.4: Distribution of eligibility age around cut-off for placebo sample: Non-AFP bankruptcies



Notes: The figure shows the distribution of age (in months; defined as in equation 1) around the individual eligibility cut-off. P-value is calculated using the discrete density test of Frandsen (2017). The sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

Figure A.5: Graphical evidence of placebo estimates of cumulative outcomes (in $1,000): Non-AFP bankruptcies



Notes: The figures show unrestricted means for each age-bin of labor market earnings and social insurance benefit take-up in $1,000 between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Placebo sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

Appendix B Old-age pension benefit calculation

In this Appendix, we outlay the details of how the old-age pension benefit levels are calculated in the Norwegian pension system. Except for the "AFP top-up" of about \$2,300, the AFP benefit calculation was equivalent to this calculation. The old-age pension benefits consist of three main pillars: a guarantee pension, an income-related pension and a defined-contribution employer-provided pension plan.

Guarantee pension Individuals who had resided in Norway for at least three years between ages 16–66 were entitled to the minimum *guarantee pension*. However, the guarantee pension was *pro-rata* cut with years of residence succeeding 40 years. A full guarantee pension in 2015 was approximately \$15,500, and the guarantee pension is indexed annually.[24]

Income pension The income pension was a mapping based on the 20 best years of income after the introduction of *Folketrygden* in 1967.[25] The mapping was based on a *base level* that we denote G, which is set by the government and indexed annually. In 2015, $1G$ was approximately \$10,000. Essentially, accrual in a year was calculated as the income exceeding $1G$. For instance, a person earning $5G$ accrued 4 in that year. Only years where the accrual exceeded the average of the 20 best years up until that year would adjust the accrued level. The income pension on accrual was capped at $12G$ which implied, in combination with a decreasing accrual rate for income exceeding a certain threshold, that the replacement rate from the old-age pension system declined with income.[26] In the years between 1967–1991, the accrual rate of pension benefits was 45 percent of the resulting accrued number calculated as above, while in the years 1992–2011, the accrual rate was 42 percent. The average of the 20 years with the highest accrual numbers constituted the final number (*sluttpoengtallet*), which was multiplied by the accrual rate for the number of years of accrual pre-1992 and post-1992, and finally the base amount G, to determine the income pension level. As a minimum, the income pension yielded $1G$, given 40 years of residence (with similar pro-rata cut as the guarantee pension).[27]

Defined-contribution pension plan After 2006, employers had to make a mandatory minimum contribution of 2 percent of earnings of their employees to a *defined contribution* pension plan. A defined benefit scheme was allowed as an alternative, however the defined benefit plan had to be on at least the same level as the expected benefits under the defined contribution plan. Contributions were mandatory for income levels between $1G$–$12G$. Benefits were paid out as life-long annuities from claiming age.

[24] Exchange rate NOK/USD=9. There were different levels depending on marital status and the labor market status of the spouse.

[25] Folketrygden is the Norwegian law governing the social security system, known as the National Insurance Scheme. All residents are automatically member of the National Insurance Scheme.

[26] For the years 1967–1992, years with income exceeding $8G$ only gave one-third accrual for the income exceeding $8G$. For instance, a person earning $9G$ would accrue 7.33 that year ($(8-1)+1\times 0.33$). After 1992, income exceeding $6G$ would only give one third accrual. A person earning $9G$ would then get $(6-1)+3\times 0.33=6$.

[27] As an example, say an unmarried individual worked for 40 years, where 25 of those years were pre-1992. The person had a smooth income for all those years equal to $6G$, meaning that the average of the 20 best years gives an accrual of 5. The person claimed old-age pension in 2015, giving approximately:

$$\$10,000+(0.45\times 5\times 25/40\times \$10,000)+(0.42\times 5\times 15/40\times \$10,000)=\$32,000$$

This benefit would be *upward adjusted* if it was lower than the minimum guarantee pension, which for 2015 was about \$16,200 (at the regular level for married couples with one spouse claiming benefits and the other working or claiming DI).